

ESSAYS ON THE ECONOMICS OF IMMIGRATION, IMMIGRANT, AND
EDUCATION PUBLIC POLICIES

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ESSAYS ON THE ECONOMICS OF IMMIGRATION, IMMIGRANT, AND EDUCATION PUBLIC POLICIES

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This dissertation examines three topics at the intersection of the economics of immigration and the economics of education. First, I study the development of human capital among immigrants by evaluating Arizona Proposition 203 (2000) and Massachusetts Question 2 (2002), which require public school districts to provide one year of Structured English Immersion to English language learner students. Using a difference-in-differences framework, I show that for recent-arrival first-generation immigrants, the two initiatives are less effective at developing English language proficiency than previous programs, such as Transitional Bilingual Education and English as a Second Language. However, I also show new heterogeneity in relative program effectiveness in that second-generation immigrants actually benefit from Structured English Immersion.

In the second chapter, I use unique data from the Current Population Survey on education by country of origin to show that the return to foreign education among immigrants is 3.3 percent. This estimate is half the size of estimates from previous studies for two reasons. First, calculating foreign education as the difference between total education and domestic education rather than as a function of total education and age at arrival eliminates the upward bias from misattributing domestic education as foreign education. Second, excluding domestic education as an endogenous control variable removes the upward bias in the return to foreign education caused by the

negative correlation between domestic education and foreign education. The results show that foreign education is even less portable to the United States labor market than previously thought.

In the third chapter, I test whether country-level educational expenditures, pupil-teacher ratios, and student achievement should be interpreted as measures of foreign school quality. I use the United States Census and the American Community Survey to show that the three measures are associated with the return to foreign education in expected directions. However, only educational expenditures are robust to accounting for group-level correlations between the wage residuals. I also show that the three measures affect immigrants who never attended school in their countries of birth as a falsification test, which suggests that they reflect country-level unobservables rather than foreign school quality.

BIOGRAPHICAL SKETCH

Albert Yung-Hsu Liu was born in Chicago, Illinois on November 11, 1979. He was raised by his parents, his sister, his aunt, and his grandmother. He enrolled at Vanderbilt University to become a high school math teacher, but chose to major in economics, mathematics, and cognitive psychology instead after taking his first economics course during his junior year. After graduation, he was supposed to work as a consultant in Boston, but the stock market crash in 2001 led him to work at The Urban Institute's Education Policy Center as a research assistant instead. He enrolled at Cornell University in 2003 to earn his PhD in economics. Finally getting his next career step right on the first try, he will work as a Researcher at Mathematica Policy Research, Inc., on the economics of immigration and education in the Bay Area.

Dedicated to Charles Liu, Shirley Wang Liu, Josephine Liu Moerschel, Jean Liu, and
Priscilla Liu, and to immigrant educators in general.

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CHAPTER 1

LEARNING ENGLISH THE FAST WAY? THE RELATIVE EFFECTIVENESS OF STRUCTURED ENGLISH IMMERSION INITIATIVES

I. Introduction

In 1998, California voters adopted Proposition 227 to change how K–12 public school districts provide instruction to English language learner (ELL) students.¹ Two years later, Arizona voters passed a similar initiative in Proposition 203, followed by Massachusetts voters with Question 2 in 2002. The initiatives consist of two components that were intended to accelerate the development of English language proficiency. The first requires districts to use Structured English Immersion (SEI), which provides instruction in an understandable level of English without incorporating home languages.² For example, the authors of California Proposition 227 claim that “[y]oung immigrant children can easily acquire full fluency in a new language, such as English, if they are heavily exposed to that language in the classroom at an early age” (California Secretary of State 1998). The second component limits the time that ELL students can receive SEI to a period not normally to exceed one year, which increases the incentive to learn English quickly. As intended, most ELL students in the three initiative states now receive SEI. However, a key unintended consequence is that districts disregard the one year constraint because they are required to provide a program that addresses the language needs of ELL students. In 2005, over 944,000 students, representing almost one in five ELL students nationwide, received SEI subject to state initiatives that are implemented this way (hereafter referred to as SEI

¹ Equivalent terms are English learner (EL) and limited English proficient (LEP).

² SEI is also sometimes referred to as Sheltered English Immersion.

initiatives).

Do ELL students develop English language proficiency faster with the SEI initiatives than with previous programs, such as Transitional Bilingual Education and English as a Second Language? Recent evaluations of California Proposition 227 have yet to reach a consensus. Bali (2001) finds that the initiative is more effective in Pasadena Unified School District, Gordon and Hoxby (2002) show that it decreases student achievement in school-grade level statewide data, and Parrish et al. (2006) find little difference in its effectiveness in cross-sectional statewide data and longitudinal data from Los Angeles Unified School District. With such a diversity of empirical methods and conclusions, there is a clear need for additional evaluations of the SEI initiatives.

In this paper, I evaluate whether Arizona Proposition 203 and Massachusetts Question 2 are more effective at developing English language proficiency than the programs used before the initiatives. I build on the California studies with three methodological contributions. First, I use a difference-in-differences framework that compares statewide trends in English language proficiency in Arizona and Massachusetts relative to comparable trends in states without the SEI initiative; changing the unit of analysis from the district to the state sidesteps the bias from endogenous compliance with the initiative at the district level. Second, I use parent-reported English speaking ability (ESA) from the 2000 Census and the 2005, 2006, and 2007 American Community Survey as the measure of English language proficiency, which avoids the psychometric assumptions needed to compare scores from different assessments or correct for student habituation to new tests. Third, I estimate the average treatment effects among recent-arrival first-generation immigrants and young second-generation immigrants as potential ELL students, which avoids the truncation or censoring of English language proficiency due to the

exemption of less English language proficient students from assessments and due to the exclusion of fluent English proficient students from the sample.

Contrary to the claims of the electorate, I show that the Arizona and Massachusetts SEI initiatives actually decrease the parent-reported English speaking ability of recent-arrival first-generation immigrants by 0.12 to 0.15 standard deviations. Based on the average first-year development of English language proficiency, the estimates correspond to a developmental delay of 0.43 to 0.73 years of school. Thus, for the majority of ELL students, Arizona Proposition 203 and Massachusetts Question 2 are less effective than previous programs, such as Transitional Bilingual Education and English as a Second Language. However, I also show that the SEI initiatives increase the parent-reported English speaking ability of young second-generation immigrants by 0.06 to 0.18 standard deviations, which corresponds to a developmental gain of 0.90 to 2.37 years of schooling. Thus, a one size fits all instructional approach is also inappropriate: the Arizona and Massachusetts SEI initiatives accelerate, or at least do not delay, the development of English language proficiency for a substantial minority of ELL students. I speculate that this heterogeneity may be due to the greater English language proficiency and the higher school quality of young second-generation immigrants due to additional assimilation of their immigrant parents.

The remainder of this paper is structured as follows. In Section II, I describe the language instruction educational programs used by districts not subject to the SEI initiatives, the adoption and implementation of the SEI initiatives, and the previous research on the relative effectiveness of the SEI initiatives. I then describe the identification strategy and empirical framework in Section III. In Sections IV and V, I discuss the results and robustness exercises, respectively. Lastly, I conclude with the policy implications in Section VI.

II. Language Instruction Educational Programs and the SEI Initiatives

A. *Language Instruction Educational Programs for ELL Students*

Who are ELL students, and what programs do K–12 public school districts provide when they are not subject to the SEI initiatives? Districts typically determine the ELL status of a new student in two steps. First, the student reports the language(s) spoken at home on a home language survey. Second, if the home language is not English, the student takes a state-approved English language proficiency assessment, such as the Language Assessment Scales or the Woodcock Johnson Language Proficiency Battery. The student is designated ELL if the test score is less than an arbitrary level, such as the 30th or 40th percentile of the test score distribution. The student retains ELL status until a subsequent test score warrants reclassification to fluent English proficient status, at which point the student participates fully in mainstream instruction.

The recent rise in immigration, particularly from non-English speaking countries, has led to a 60 percent increase in the population of ELL students, from 3.2 million in 1995 to 5.1 million in 2005 (National Clearinghouse for English Language Acquisition and Language Instruction Educational Programs 2008). In contrast, the population of all other students (including fluent English proficient students) was roughly constant at 44.3 million over the same period. The majority of ELL students are first-generation immigrants at 53.9 percent, and of this group, 72.9 percent have been in the United States for less than five years (Anette M. Zehler, et al. 2003) The remaining 46.1 percent of ELL students are native-born individuals, who are usually second-generation immigrants (native-born with at least one foreign-born parent) in elementary school.

Districts typically exercise choice over which language instruction educational program they provide to their ELL students. The diversity of programs is due to the

Elementary and Secondary Education Act (1965), which provides financial incentives to educate ELL students without requiring specific programs. Subsequent reauthorizations changed the incentives for using particular programs, but a consistent theme is the mandate to provide some program after the Supreme Court ruled that equal resources alone do not provide ELL students with a meaningful education in *Lau v. Nichols* (1974). At present, the No Child Left Behind Act of 2001 (NCLB) (U.S. Code 2002) sets two objectives for language instruction educational programs: (1) English language proficiency and (2) adequate yearly progress in core academic content knowledge. This paper focuses on how K–12 public school districts meet the first objective.

Table 1. Language Instruction Educational Programs

	(1)	(2)	(3)	(4)
	English language arts		Other subject areas	
	Language of instruction	Peer group	Language of instruction	Peer group
English as a Second Language	English	All ELL students	English	All students
Structured English Immersion	Understandable English	All ELL students	Understandable English	All ELL students
Transitional Bilingual Education	Home language to English	ELL students with same home language	Home language to English	ELL students with same home language

Source: Adapted from National Clearinghouse For English Language Acquisition and Language Instruction Educational Programs (2007).

The three most common programs are summarized in Table 1. The first is English as a Second Language, which is based on the pedagogical theory that the development of English language proficiency requires maximum exposure to the English language (Rosalie Pedalino Porter 1996). The instructional response of this program is to provide (1) specific periods of English language arts instruction without

using home languages and (2) instruction in other subject areas in mainstream classrooms with instruction in English. The second program, which is based on the same pedagogical theory, is SEI. In this program, ELL students receive instruction in an understandable level of English for all subjects, which takes place in self-contained classrooms with all ELL students regardless of home language. The third program is Transitional Bilingual Education, which is sometimes referred to as Early-Exit Transitional Bilingual Education. The pedagogical theory of this approach is the developmental interdependence hypothesis, which claims that children with low home language ability and no English language ability are cognitively unreceptive to instruction exclusively in English (James Cummins 1979). The instructional response is to provide instruction in home languages until instruction in English becomes accessible through the transfer of general language skills. The shift in the language of instruction is expected to take two to three years and occurs in self-contained classrooms with ELL students with the same home language. Prior to the three SEI initiatives, English as a Second Language and Transitional Bilingual Education were the two most commonly used programs by K–12 public school districts, with Transitional Bilingual Education being more common in elementary school grade levels (Diane August and Kenji Hakuta 1997; Anneka L. Kindler 2002).

It is worth pointing out that there are multiple differences between these programs. SEI differs from Transitional Bilingual Education in the language of instruction (understandable English versus home language and English) and the composition of the ELL student peer group (any home language versus same home language). SEI differs from English as a Second Language in the difficulty of the English used in instruction (understandable versus mainstream) and the composition of the peer group (ELL students versus mainstream students). There may be other program differences, such as teacher quality, the use of teacher aides, and class size.

Previous evaluations typically compare overall differences in program effectiveness rather than the effectiveness of a specific component holding all else equal, which makes it difficult to know the true drivers of program effectiveness (Diane August and Kenji Hakuta 1997).

The optimal program is the one that helps districts maximize the educational objectives defined in NCLB subject to the district budget constraint. In Arizona, local choice prior to Proposition 203 resulted in 25.3 percent of ELL students receiving instruction that incorporated home languages (Anneka L. Kindler 2002). More direct program data from the Arizona Department of Education indicates that 36.7 percent of ELL students received Transitional Bilingual Education (Lisa G. Keegan 1999).³ There are two reasons why these programs, and locally chosen programs in general, may not be optimal programs. First, scholars have been unable to reach a consensus on the relative effectiveness of the programs described above; see August and Hakuta (1997) and Rossell and Baker (1996b) for reviews. Thus, district administrators and teachers lack access to objective, consensus-based evidence when they choose which programs to provide. Second, there is an incentive problem for districts to switch to better programs. Tenured teachers may be unwilling to change to more effective programs if they are not accountable for the English language proficiency of their ELL students and if the change requires costly retraining and recertification.

As a consequence of these concerns, some districts are required to use specific programs due to interventions by state legislatures. In 1971, Massachusetts became

³ This includes K–6 Transitional Bilingual Programs, 7–12 Secondary Bilingual Programs, and K–12 Bilingual-Bicultural Programs. It excludes English as a Second Language Programs and Individual Education Programs.

the first state to require the use of Transitional Bilingual Education when there are at least 20 ELL students with the same home language in a district; all other ELL students receive English as a Second Language.⁴ Thus, prior to the adoption of Question 2, 85.3 percent of Massachusetts ELL students in 1999 received instruction that incorporates home languages, dramatically more than the 25.3 percent in Arizona (Anneka L. Kindler 2002). The dissimilarity of the program distributions in Arizona and Massachusetts prior to the initiatives is useful because it provides two different counterfactuals when I estimate the relative effectiveness of the SEI initiatives.

B. Adoption and Implementation of the SEI Initiatives

State legislation is not the only way to override the local choice of a suboptimal program, and it is also not the final word on the issue. Of the 24 states that allow voters to set policy through the initiative process, three used it to require K–12 public school districts to provide ELL students (1) with SEI (2) for a period not normally to exceed one year. California adopted Proposition 227 with 60.9 percent of the vote in 1998. In 2000, Arizona followed suit with Proposition 203 with 74.7 percent of the vote, and in 2002, Massachusetts adopted Question 2 with 68.0 percent of the vote. One other state, Colorado, considered the initiative in 2002 as Amendment 31 but rejected it with 56.2 percent of the vote. The sponsors of the initiatives argued that district administrators, teachers, and state legislatures were unwilling to do what is best for ELL students, as shown by the “slow” 6.7 percent annual transition rate from ELL to fluent English proficient status (Alice Callaghan, Ron Unz and Fernando Vega 1998).

The adoption of the initiatives in three of the four states is due to demographic-

⁴ See Rossell and Baker (1996a) for a historical account.

driven differences in preferences over the language of instruction. Robinson, Rivers and Brecht (2006) show that people of color and speakers of a second language were less likely to support English only language instruction educational programs in the 2000 General Social Survey. Furthermore, a California exit poll shows that Hispanic voters were 22 percentage points less likely to support Proposition 227 (Valentina A. Bali 2008). These studies show that the parents of ELL students were less likely to support the SEI initiatives, but since they make up a small portion of the electorate, it is not surprising that the initiatives were adopted in California, Arizona, and Massachusetts. Anecdotal evidence suggests that the unique outcome for Colorado was due to a large campaign contribution and a series of uniquely effective campaign advertisements (Nancy Mitchell 2002).

How do K–12 public school districts implement the two components of the SEI initiatives? On the first part, most districts in the three initiative states now provide SEI. However, there is substantial variation by state due to the availability of waivers for alternative programs. For example, California Proposition 227 allows a parent to file a waiver if the ELL child already knows English, is older than ten years old, or has special needs that would be better addressed with a different program. If the waiver is granted by the district, then the ELL student can participate in another program provided by the district. As shown in Figure 1, only about 50 percent of California ELL students participated in SEI between 1999 (when the state started to collect information on its use) and 2007. While some parents initiate waivers, many are encouraged to do so by district administrators and teachers (Christine H. Rossell 2003). This suggests that there is district-level endogenous compliance with the initiative. Indeed, Gordon and Hoxby (2002) and Parrish et al. (2006) show that the schools that complied with the initiative have ELL students with higher initial English language proficiency and lower shares of ELL students, characteristics that are

associated with ELL students who may be easier to teach.

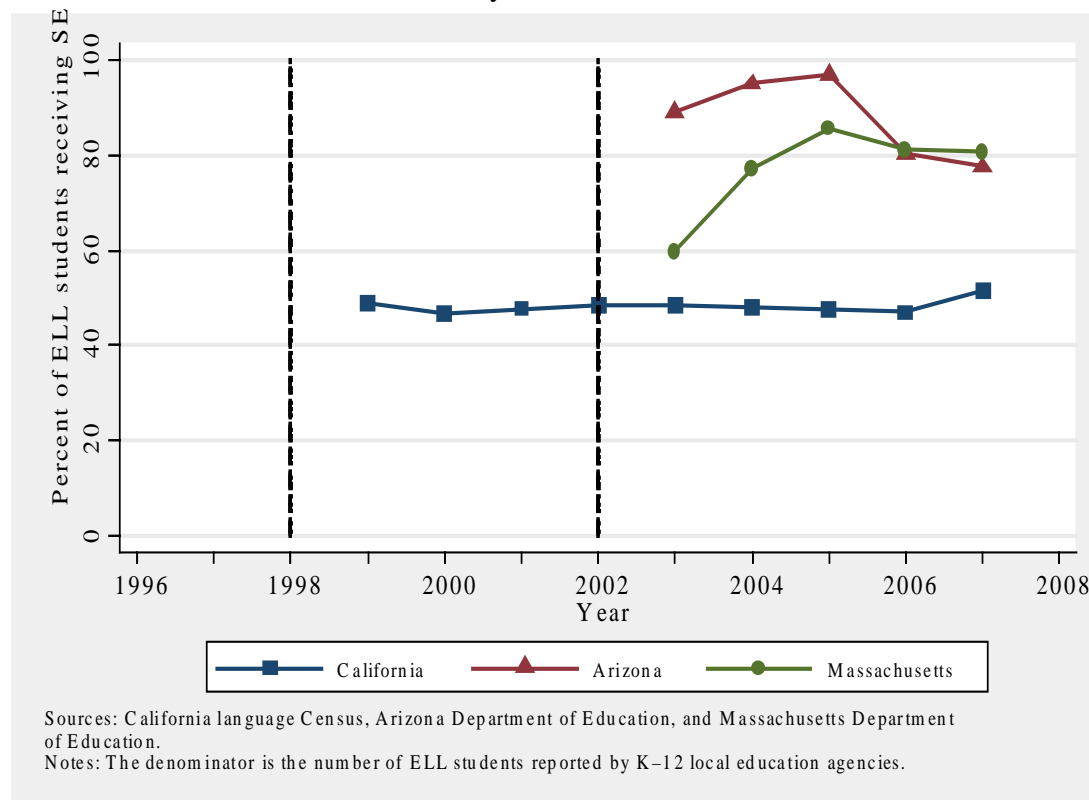


Figure 1. Program Compliance with the SEI Initiative by State

In contrast, Arizona and Massachusetts implemented stricter versions of the SEI initiative. Waiver eligibility in Arizona is limited to ELL students who score above the 50th percentile of non-ELL test scores (Kate S. Mahoney, Marilyn S. Thompson and Jeff MacSwan 2004), which generates compliance rates of 80 to 95 percent because eligible students already participate in mainstream instruction. Compliance rates in Massachusetts are similarly high, with the exception of a handful of Two-Way Bilingual Education programs exempted by the state legislature in 2003.

The second component restricts program participation to a period not normally to exceed one year, which is intended to increase the incentive for ELL students to learn English quickly. A key feature of the initiatives in practice is that this component is not enforced under guidance from the state departments of education.

For example, the California Department of Education (1998) interprets one year as the minimum rather than the maximum enrollment in SEI. Similarly, the Massachusetts Department of Education (2006) states that Question 2 may not be interpreted as a limit on the time spent participating in SEI. Three case studies in Massachusetts show that districts do interpret one year as the target rather than as a binding constraint (Ester J. de Jong, Mileidis Gort and Casey D. Cobb 2005). The guidelines are motivated by federal law that requires K–12 public school districts to provide a program to ELL students to address their language needs (1974). Thus, while ELL students are not limited to one year of program participation, they are largely limited in only receiving SEI rather than Transitional Bilingual Education or English as a Second Language.

The difference between the adopted initiative and the implemented initiative has an important public choice implication. A common argument in favor of setting policy through the initiative process is that if the initiative is adopted, it represents an improvement to the median voter (John G. Matsusaka 2005). State attorneys general typically invalidate initiatives that are legally unsound so that those voted on by the electorate can actually be implemented. But in the case of the SEI initiatives, the one year restriction was disregarded under the guidance of state departments of education. Thus, if voters based their decisions in part on the one year restriction, the implemented SEI initiative may not be one that is preferred by the median voter.

One final issue is the implementation of additional policies following the adoption of Arizona Proposition 203. Like the other two initiative states, the Arizona Department of Education recommends that ELL students continue to receive SEI if they are not yet fluent English proficient after one year, but legislation in 2006 limits state funding for ELL students to two years to encourage districts to develop English language proficiency as quickly as possible (Arizona Department of Education 2006).

Because the data in this paper only goes through 2007, this funding change does not apply to the students in the samples described below. There are two other policies that may affect the development of English language proficiency. First, Arizona reduced the number of course credits for teacher certification in SEI from twenty one to four, and second, it doubled funding for ELL course materials and teacher recertification expenses (Kate S. Mahoney, Jeff MacSwan and Marilyn S. Thompson 2005).

Because these two policies were adopted as a consequence of Arizona Proposition 203, I interpret them as part of the overall effect of the initiative on English language proficiency.

C. Previous Research on the SEI Initiatives

Previous evaluations focus on California because it was the first state to adopt and implement the SEI initiative. The three studies I discuss in this subsection demonstrate the difficulties of evaluating the initiatives and language instruction educational programs in general.

The first evaluation by Bali (2001) estimates the effect of California Proposition 227 on ELL student achievement in Pasadena Unified School District. The paper shows that reading score deficits on the Stanford Achievement Tests among ELL students receiving Transitional Bilingual Education disappear after they are subject to the SEI initiative. The paper is unique in considering the censoring of student achievement by modeling test exemptions for less English language proficient ELL students with a Heckman selection model. However, there are some important limitations that cast doubt on its conclusion that the SEI initiative accelerated the development of English language proficiency relative to Transitional Bilingual Education. While the paper focuses on the left-censoring of the dependent variable, it does not consider the truncation on the right due to the exclusion of fluent English proficient students from the sample. In addition, the assumption of exogenous

compliance to the initiative is unreasonable because the districts (and schools) that comply with California Proposition 227 differ from the average district by having more financial resources, greater community support for the initiative, and ELL students who are easier to teach.

A second evaluation by Gordon and Hoxby (2002) estimates the effect of the California SEI initiative on school-grade average test scores and shows that it decreases language, math, and reading achievement in early grades. One advantage of the paper over Bali (2001) is its coverage of the entire state rather than a particular district. The main specification is a regression of the change in average test scores on the change in the share of ELL students who received Transitional Bilingual Education. The paper is unique in addressing endogenous compliance with the SEI initiative by instrumenting for the actual change in the Transitional Bilingual Education enrollment share with the change in the enrollment share if the district mechanically complied with the initiative. However, the paper suffers from two limitations. First, the paper does not address the truncation of test scores that generate the school-grade averages test scores. Second, the paper imposes strong psychometric assumptions to purge the data of habituation bias; California's switch to the Stanford Achievement Test in 1997 means that part of the change in test scores is due to students learning how to take the new test rather than any actual change in the quality of instruction.

The third study is the evaluation mandated by the California state legislature. Like Bali (2001), Parrish et al. (2006) also study the relative effectiveness of the initiative in a specific school district, in this case, Los Angeles Unified School District. Using a value-added methodology, the paper shows little evidence that the SEI initiative changed student achievement, but again, this conclusion is not surprising given the truncation of the test score data. In a separate analysis, the authors estimate

the relative effectiveness of the initiative using cross-sectional statewide data. However, this approach suffers from endogenous compliance with the initiative and the truncation of student achievement. Thus, given these methodological limitations, there is still much we do not know the relative effect of the SEI initiative on the English language proficiency of ELL students.

III. Identification Strategy and Empirical Framework

A. Identification Strategy and Outcome Measure

The ideal research design to evaluate the SEI initiatives is to randomly assign the initiative to districts within a state and compare the English language proficiency in districts treated with the initiative with districts not treated with the initiative. The measure of English language proficiency would come from a single assessment that students do not “learn” how to take (i.e. no habituation). Lastly, the sample would consist of ELL students and former ELL students and would not exempt less English proficient students from the test. Unfortunately, evaluating the SEI initiatives – and language instruction educational programs in general – in practice is difficult for three reasons. First, district-level compliance with the initiative is endogenous. Second, the assessments used to measure English language proficiency differ between states and within states over time. Third, most datasets truncate or censor English language proficiency on the left by exempting low English proficient students from taking tests and on the right by not testing fluent English proficient students who are no longer considered ELL.

In this paper, I use a straightforward empirical strategy to overcome these difficulties. To address the first concern, I change the unit of analysis from the district to the state. This empirical strategy is useful because the compliance rates in Arizona and Massachusetts are high and because endogenous non-compliance by districts will only lead to a conservative average treatment effect estimator. I employ a quasi-

experimental nonequivalent control group research design in the form of the standard difference-in-differences econometric approach. Arizona and Massachusetts are the treatment groups, 2000 is the pre period and 2005, 2006, and 2007 are the post period. The control groups in the primary specifications are neighboring states in the Other West for Arizona and in the Other Northeast for Massachusetts.⁵ I exclude California from the Other West control group because the ELL students in the state have partial exposure to Proposition 227, and their inclusion would bias the average treatment effect estimator toward zero. The strength of the difference-in-differences framework is that it eliminates state-invariant trends such as changes in the composition of ELL students due to tightening federal immigration restrictions as part of the War on Terror and changes in the behavior of districts, schools, and teachers in response to NCLB. The approach also differences out selective emigration if less English language proficient immigrants are more likely to return to their home countries.

The second issue of non-comparable measures of English language proficiency is due to the changing set of acceptable assessments both between states and within states over time. Rather than standardize scores from different assessments or correct for habituation to new tests, I employ an alternative strategy of using parent-reported English speaking ability from the Census and American Community Survey as the measure of English language proficiency. Since 1980, the Census and the American Community Survey asks household respondents whether each household member

⁵ The West region consists of Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming. The Northeast region consists of Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont.

speaks English “at home,” and if not, whether the household member speaks English “very well,” “well,” “not well,” or “not at all.” Since respondents are typically parents, I interpret English speaking ability as parent-reported for children and self-reported for adults.

The primary concern with parent-reported English speaking ability as a policy outcome is that it may not be a meaningful measure of English language proficiency. On the one hand, previous studies argue that self-reported English speaking ability is a good measure of English language proficiency because it is positively associated with immigrant educational attainment, occupational choice, and wages (Hoyt Bleakley and Aimee Chin 2004; Sherrie A. Kossoudji 1988; Walter McManus, William Gould and Finis Welch 1983). On the other hand, the measure may capture other aspects of human capital that are omitted from the empirical specification. While there is evidence that it proxies for other aspects of English language proficiency, such as understanding and writing ability (Anthony P. Carnevale, Richard A. Fry and B. Lindsay Lowell 2001; Barry R. Chiswick 1991), self-reported English speaking ability may also be associated with unobserved non-language aspects of human capital which makes its interpretation difficult. The best available evidence of its validity is its positive correlation with standardized test scores in Census validation studies (Robert Kominski 1989). These studies have led some scholars to use self-reported English speaking as a policy outcome when studying the effect of the language of instruction received as a child on the self-reported English speaking ability as an adult (Joshua D. Angrist, Aimee Chin and Ricardo Godoy 2008). However, it is difficult to say whether household respondents provide inflated or harsher judgments of English speaking ability when it comes to their children versus themselves.

To validate subjective parent-reported English speaking ability as a meaningful measure of English language proficiency, I turn to data from the 2003-1 cohort of the

New Immigrant Survey. This survey contains data on immigrants who were recently granted legal permanent residence in the United States. I focus on the subsample of adults who were asked the Census and American Community Survey questions on English speaking ability and link them to their children who took four tests from the Woodcock-Johnson III Tests of Achievement: Letter-Word Identification, Passage Completion, Calculation, and Applied Problems. I am particularly interested in the relationship between parent-reported English speaking ability and objective test scores from the Letter-Word Identification and Passage Completion tests because as recently as 2004, the two tests were part of a battery approved by the Arizona Department of Education to assign ELL status to students (Kate S. Mahoney, Jeff MacSwan and Marilyn S. Thompson 2005).

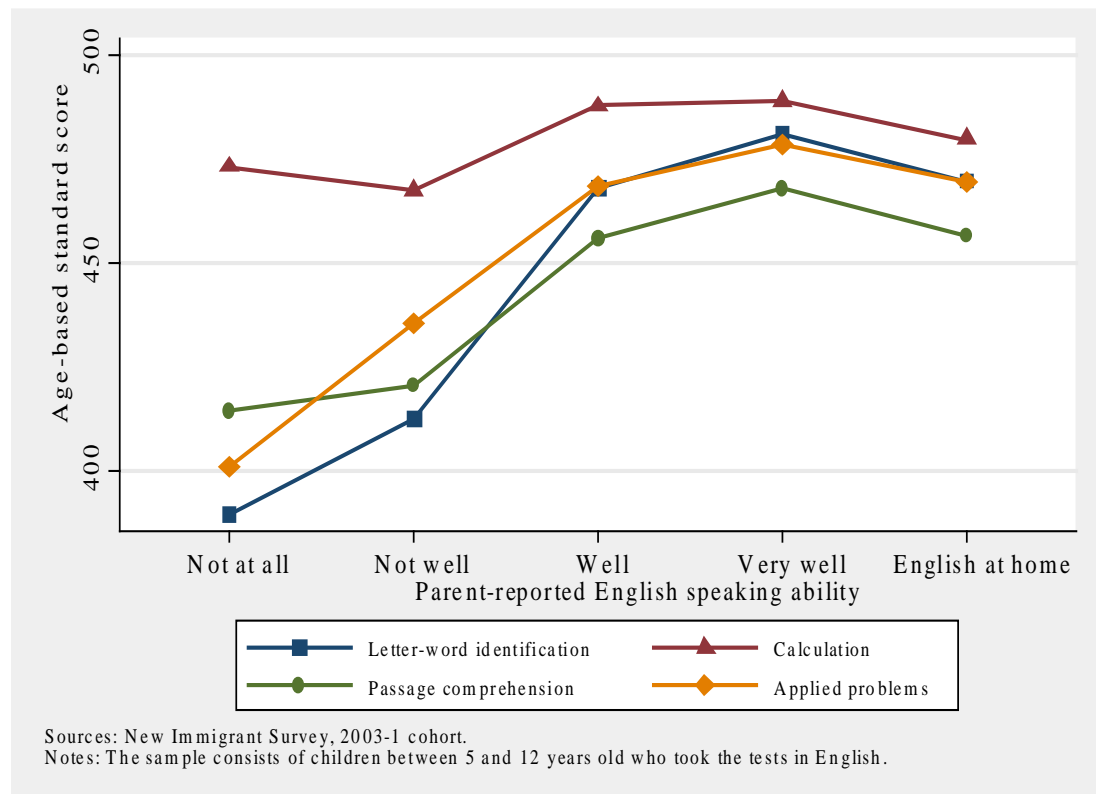


Figure 2. Parent-Reported ESA and Woodcock-Johnson III Test Scores

Figure 2 presents average raw test scores for the four tests at each value of

parent-reported English speaking ability. The top line with the square markers shows that children with higher parent-reported English speaking ability also have higher Letter-Word Identification test scores. The line with the circle markers shows a similar upward pattern for Passage Completion. Thus, subjective parent-reported English speaking ability is positively associated with two objective measures of English language proficiency that districts use to determine ELL status. It is useful to contrast these trends with those of the two math tests. The line with the triangle markers for Calculation shows a weak association with parent-reported English speaking ability, and the line with the diamond markers for Applied Problems shows a positive relationship with parent-reported English speaking ability. The trends for the two math tests support the conclusion that parent-reported English speaking ability is a meaningful measure of English language proficiency because the Calculation test is an arithmetic exam in a common mathematical language across countries, and the Applied Problems test is a set of word problems that requires English language proficiency to complete correctly.

The trend lines also shed light on the cardinality of parent-reported English speaking ability. Previous studies typically combine the speaks English “at home” group with the speaks English “very well” group and then impose a linear specification of self-reported English speaking ability, with each additional level representing an equal increase in English language proficiency. The figure shows that a linear specification is supported by the data and that the speaks English “at home” group can be combined with speaks English “well,” “very well,” or both groups. In this paper, I combine the speaks English “at home” and the speaks English “very well” categories and impose a linear specification, although I relax these assumptions in the robustness exercise section.

The third and final methodological concern is the truncation and censoring of

English language proficiency. Districts often exempt the lowest English proficient students from assessments tests and stop testing fluent English language proficient students, which makes it difficult to find any difference in program effectiveness.⁶ My solution is to identify two groups of potential ELL students in the 2000 Census and the 2005, 2006, and 2007 American Community Survey public use microdata samples. The first group consists of recent-arrival first-generation immigrant children, attending K–12 public schools, and not living in group quarters. I define recent-arrival as less than three years since migration so that the average treatment effects are based on students always subject to the initiative or never subject to the initiative. To abstract from the potential effect of the initiatives on school dropout status, I restrict the sample to respondents between 8 and 16 years old so that every child is subject to state compulsory schooling laws.

The second sample consists of young second-generation immigrant children, attending K–12 public school, and not living in group quarters. To unambiguously identify second-generation immigrants, I restrict the sample to native-born children with two foreign-born parents. As with recent-arrival first-generation immigrants, I limit the sample to children always subject to or never subject to the initiatives. This means that I focus on children between 8 and 11 years old so that they only have (roughly) three years of attendance in public schools.

⁶ For example, an alternative objective measure of English language proficiency that could be used with my identification strategy is reading test scores from the National Assessment of Educational Progress (NAEP). However, the NAEP only started collecting data on former ELL students in 2005, and it exempts between 20 to 25 percent of students from the assessments between 1998 and 2005.

The advantage of these samples over those of previous studies is that I do not gain or lose students as they develop their English language proficiency. The least English proficient students are still in the sample due to the outreach of the Census and the American Community Survey to count households who do not speak English. And, since first-generation and second-generation immigrant statuses are fixed attributes, I do not exclude any students once they become fluent English proficient.

An additional feature of these samples is that the average treatment effect estimator is based on students consistently treated with the SEI initiatives and students consistently treated with previous programs. One limitation of previous evaluations is that the effect of the initiative may in fact be due to the disruption in the language of instruction rather than the change in the language instruction to English. Because the two samples exclude students with partial exposure to each policy regime, the average treatment effect estimator cannot be interpreted as the effect of an instructional disruption.

B. Econometric Framework

The primary econometric specification is Equation (1) for student i , born in source country c , with home language l , and living in neighborhood n in experimental group s in period t . I use sample weights for the five percent 2000 sample as is, and I pool and reweight the one percent 2005, 2006, and 2007 American Community Survey samples (hereafter referred to as the 2005 sample) into a single year. I use sample weights for the five percent 2000 sample as is, and I pool and reweight the one percent 2005, 2006, and 2007 American Community Survey samples (hereafter referred to as the 2005 sample) into a single year.⁷ The dependent variable

⁷ Specially, I reweight each observation so that total weight in each of 2005, 2006, and

is parent-reported English speaking ability, , which takes on values of speaks English (3) “at home” or “very well,” (2) “well,” (1) “not well,” and (0) “not at all.”

Unfortunately, the data do not include historical values of parent-reported English speaking ability, so I cannot use a value-added methodology that controls for initial English language proficiency. The average treatment effect estimator that gives the relative effectiveness of the SEI initiatives is β_3 . If $\hat{\beta}_3 > 0$, then the SEI initiatives are associated with greater English language proficiency than previous programs; if $\hat{\beta}_3 < 0$, then previous programs such as Transitional Bilingual Education and English as a Second Language are more effective than the SEI initiatives.

$$ESA_{iclnst} = \beta_0 + Treatment_s \beta_1 + Post_t \beta_2 + Treatment_s * Post_t \beta_3 + \mathbf{X}_{iclnst} \mathbf{\Gamma} + Y_{nst} \delta_1 + CO_{lnst} \delta_2 + \Theta_c + \varepsilon_{iclnst} \quad (1)$$

I include student and household control variables in \mathbf{X} to compare students with similar observable characteristics. The student control variables for both samples include sex and race. For recent-arrival first-generation immigrants, I also control for years since migration dummy variables and age at arrival (quadratic); for young second-generation immigrants, I also control for age dummy variables. The household controls are the natural log of household income in 2000 dollars, the head’s highest grade completed, and household size.⁸ For young second-generation immigrants, I also control for the head’s years since migration (quadratic). In some specifications, I control for the head’s English speaking ability, although this variable is endogenous if children teach English to their less fluent parents. Lastly, I control for family

2007 contributes one-third of the average total weight across all three years.

⁸ The family interrelationship variables are from the Integrated Public Use Microdata Series (Steven Ruggles, et al. 2008). I standardize income across surveys so that the top-codes are the same in 2000, 2005, 2006, and 2007 for each type of income.

composition with dummy variables for single mother, single father, and no parent, with two-parent families as the omitted category.

The ideal empirical specification would control for district, school, and teacher characteristics, but the smallest identifiable area in the public use Census and the American Community Survey is the public use microdata area, which typically contains 100,000 people. Thus, the best available proxy for school quality is the natural log of neighborhood median household income in 2000 dollars, where the neighborhood is defined as the public use microdata area. I also control for the neighborhood home language concentration, which measures the cost of maintaining home languages (Edward P. Lazear 1999). I impute the home language for immigrants that speak English “at home” because many continue to be fluent in the language spoken at arrival. The imputation is based on the modal language among immigrants from the same country with one year since migration in the 2000 Census. I set this variable equal to zero and a dummy variable to one for English home language status, which makes the effect of neighborhood home language concentration due to non-English home languages only.

For recent-arrival first-generation immigrants, I also include source country fixed-effects for countries with at least five observations in the treatment and control groups in both 2000 and 2005 and assign the remaining immigrants to five world region dummy variables. I use similarly constructed source country fixed-effects for the household head in the young second-generation immigrant sample. This allows me to partially control for fixed, unobservable characteristics by source country that are correlated with English speaking ability. I also experiment with alternative specifications that replace country fixed-effects with similarly constructed language fixed-effects. In these specifications, I control for Chiswick and Miller’s (2005) measure of linguistic distance from the English language. The home language

variables are based on the imputation strategy described above, and I set the linguistic distance variable equal to zero if the home language is English. Lastly, I cluster standard errors to allow for an arbitrary correlation of error terms at the state-year level.

Table 2. Sample Statistics for Recent-Arrival First-Generation Immigrants in 2000

	(1)	(2)	(3)	(4)
	Arizona and Other West		Massachusetts and Other Northeast	
	Treatment	Control	Treatment	Control
Parent-reported ESA	1.774 (1.037)	2.035 (0.954)	2.189 (0.915)	2.188 (0.918)
Zero YSM	0.089 (0.284)	0.106 (0.308)	0.079 (0.270)	0.091 (0.287)
One YSM	0.509 (0.500)	0.488 (0.500)	0.534 (0.499)	0.494 (0.500)
Two YSM	0.402 (0.491)	0.406 (0.491)	0.387 (0.488)	0.415 (0.493)
Ln income	9.707 (2.313)	10.062 (1.853)	9.828 (2.419)	9.838 (2.384)
Head education	9.310 (4.824)	11.074 (4.883)	11.939 (4.109)	11.819 (4.279)
Head self-rep. ESA	1.411 (1.120)	1.735 (1.082)	1.853 (1.050)	1.930 (1.059)
N	595	1882	623	3807
Weighted N	14023	51316	15561	102533

Sources: 2000 Census.

Notes: Standard deviations are in parentheses. The sample consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration. The West excludes California. The West excludes California.

Table 2 presents selected sample means and standard deviations for recent-arrival first-generation immigrants before the initiatives in 2000. There are some statistically significant differences in observables between the treatment and control group in Arizona and Other West. For example, both children and their household heads have lower English speaking ability in Arizona than in Other West. In both

regions, however, parents rate their children as having higher English speaking ability than they do for themselves. In addition, Arizona recent-arrival first-generation immigrants have household heads with almost two years less education and have lower household income than their counterparts in Other West. In contrast, there are few differences in observables between the treatment and control group in Massachusetts and Other Northeast.

Table 3. Sample Statistics for Young Second-Generation Immigrants in 2000

	(1)	(2)	(3)	(4)
	Arizona and Other West		Massachusetts and Other Northeast	
	Treatment	Control	Treatment	Control
Parent-reported ESA	2.598 (0.650)	2.710 (0.561)	2.799 (0.457)	2.801 (0.487)
Eight years old	0.294 (0.456)	0.289 (0.453)	0.237 (0.426)	0.264 (0.441)
Nine years old	0.276 (0.447)	0.273 (0.446)	0.326 (0.469)	0.260 (0.438)
Ten years old	0.254 (0.436)	0.225 (0.418)	0.226 (0.418)	0.250 (0.433)
Ln income	10.250 (1.418)	10.500 (1.249)	10.717 (1.097)	10.564 (1.524)
Head education	9.435 (4.620)	9.935 (4.899)	11.679 (4.383)	12.071 (4.106)
Head self-rep. ESA	1.841 (0.999)	2.021 (0.889)	2.213 (0.881)	2.288 (0.837)
N	1385	3801	1080	8449
Weighted N	35615	99651	25085	227520

Sources: 2000 Census.

Notes: Standard deviations are in parentheses. The sample consists of second-generation immigrants, attending public school, not living in group quarters, and eight to ten years old. The West excludes California.

Table 3 shows that there are fewer differences in observables between the treatment and control groups in the young second-generation immigrant sample. For example, the differences in parent-reported English speaking ability and the head's

self-reported English speaking ability by experimental group in Arizona and Other West are of smaller magnitude, and household income is similar across experimental groups in both regions. Notably, parents continue to rate their children as having higher English speaking ability than they do for themselves. Lastly, the household income and the head's self-reported English speaking ability of young second-generation immigrants are greater than those of recent-arrival first-generation immigrants, which is due to their greater economic assimilation with more years since migration to the United States.

IV. The Effects of the Arizona and Massachusetts SEI Initiatives

Table 4 presents the basic average treatment effect estimates without any control variables for recent-arrival first-generation immigrants and young second-generation immigrants. The upper left panel shows that the mean English speaking ability in Arizona is lower than that of nearby states in both 2000 and 2005. The average treatment effect of the Arizona SEI initiative on parent-reported English speaking ability is -0.081 but is not statistically significant. In contrast, the upper right panel for the Massachusetts and Other Northeast shows that while the mean English speaking ability is the same in the treatment and control group in 2000, it is lower in the treatment group in 2005. The average treatment effect of the Massachusetts SEI initiative is -0.191 units, which corresponds to a decrease of 0.21 standard deviations of parent-reported English speaking ability. The results provide mixed evidence that there is a negative effect of the SEI initiatives on English language proficiency, with no difference in the effectiveness of Arizona Proposition 203 and a lower effectiveness of Massachusetts Question 2. To the extent that most ELL students in Massachusetts received Transitional Bilingual Education prior to the SEI initiative, this means that recent-arrival first-generation immigrant students have higher English language proficiency with Transitional Bilingual Education than they do with SEI.

Table 4. Baseline Average Treatment Effects on Parent-Reported ESA

	(1)	(2)	(3)	(4)	(5)	(6)
	Arizona and Other West			Massachusetts and Other Northeast		
	2000	2005	Difference	2000	2005	Difference
Recent-Arrival First-Generation Immigrants						
Treatment	1.774** (0.046) [595]	1.784** (0.072) [239]	0.010 (0.086) [834]	2.189** (0.040) [623]	1.953** (0.063) [233]	-0.236** (0.075) [856]
Control	2.035** (0.025) [1882]	2.125** (0.038) [777]	0.091** (0.045) [2659]	2.188** (0.017) [3807]	2.143** (0.030) [1266]	-0.045 (0.035) [5073]
Difference	-0.261** (0.053) [2477]	-0.341** (0.081) [1016]	-0.081 (0.097) [3493]	0.001 (0.044) [4430]	-0.190** (0.070) [1499]	-0.191** (0.082) [5929]
Young Second-Generation Immigrants						
Treatment	2.576** (0.024) [1133]	2.782** (0.018) [776]	0.205** (0.030) [1909]	2.793** (0.017) [855]	2.870** (0.024) [421]	0.077** (0.029) [1276]
Control	2.692** (0.012) [3004]	2.780** (0.013) [2017]	0.088** (0.018) [5021]	2.795** (0.007) [6521]	2.846** (0.011) [3782]	0.051** (0.013) [10303]
Difference	-0.116** (0.026) [4137]	0.002 (0.022) [2793]	0.117** (0.034) [6930]	-0.002 (0.018) [7376]	0.024 (0.026) [4203]	0.026 (0.032) [11579]

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * $p < .10$, ** $p < .05$. Standard errors are in parentheses. Sample sizes are in brackets. The sample in the top panel consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration unless otherwise specified. The sample in the bottom panel consists of second-generation immigrants, attending public school, not living in group quarters, and eight to ten years old unless otherwise specified. The West excludes California.

The results for young second-generation immigrant students show very different effects of the SEI initiatives. To the best of my knowledge, this paper is the first to decompose the average treatment effect of a language instructional educational program by nativity. In the bottom left panel, the results indicate that while the parent-reported English speaking ability of second-generation immigrants was lower

in 2000, by 2005 they were the same. The average treatment effect is 0.117 units, which is equal to 0.18 standard deviations of parent-reported English speaking ability. In contrast, the results for Massachusetts show no such effect, with the average parent-reported English speaking being the same between treatment and control groups in both 2000 and 2005. To the extent that young children are more likely to receive Transitional Bilingual Education, the results imply that young second-generation immigrants have higher English language proficiency with the SEI initiatives than with Transitional Bilingual Education, at least in Arizona.

To ensure that I compare the parent-reported English speaking ability of children with similar observables, Table 5 presents estimates for recent-arrival first-generation immigrants that control for child, household, and neighborhood characteristics. Column (1) for the Arizona and Other West and (4) for the Massachusetts and Other Northeast are the preferred specification which controls for sex, race, years since migration dummy variables, age at arrival (quadratic), household income, head's highest grade completed, household size, family composition, neighborhood median household income, neighborhood home language concentration, and source country fixed-effects. The average treatment effect estimate of the Arizona SEI initiative is -0.125, which is close to marginal statistical significance. In contrast, the coefficient estimate for Massachusetts shows that the initiative decreases parent-reported English speaking ability by 0.138 units. When I convert the coefficient estimates into effect sizes, Arizona Proposition 203 and Massachusetts Question 2 decrease parent-reported English speaking ability by 0.12 and 0.15 standard deviations, respectively.

The coefficient estimates for the control variables are consistent with those of the literature. The first year of residence in the United States is associated with an increase of 0.292 to 0.188 units of parent-reported English speaking ability, and I use

these coefficient estimates to transform to the average treatment effect into a developmental delay of 0.43 to 0.73 years for the Arizona and Massachusetts SEI initiatives, respectively. The effects of household characteristics operate in expected directions: the head's highest grade completed and household income are both associated with greater parent-reported English speaking ability.

Table 5. The Effect of the Initiatives on the ESA of First-Generation Immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
	Arizona and Other West			and Other Northeast		
Treatment	-0.037 (0.053)	-0.062 (0.040)	-0.049 (0.052)	-0.013 (0.036)	0.005 (0.028)	-0.009 (0.035)
Post	0.034 (0.076)	0.103* (0.056)	0.040 (0.077)	-0.020 (0.041)	0.003 (0.036)	-0.035 (0.039)
Treatment*	-0.125 (0.084)	-0.150** (0.060)	-0.133 (0.083)	-0.138** (0.046)	-0.078* (0.041)	-0.148** (0.042)
Child Characteristics						
One YSM	0.292** (0.089)	0.261** (0.071)	0.292** (0.091)	0.188** (0.085)	0.225** (0.085)	0.198** (0.084)
Two YSM	0.576** (0.079)	0.566** (0.063)	0.569** (0.080)	0.461** (0.099)	0.499** (0.100)	0.469** (0.096)
Household Characteristics						
Ln income	0.030** (0.012)	0.008 (0.014)	0.030** (0.012)	0.019** (0.005)	0.006 (0.007)	0.020** (0.005)
Head education	0.027** (0.006)	0.003 (0.008)	0.030** (0.006)	0.028** (0.003)	0.005* (0.003)	0.028** (0.003)
Head self-rep. ESA		0.354** (0.035)			0.321** (0.015)	
Child FE:	Country	Country	Language	Country	Country	Language
R ²	0.240	0.354	0.233	0.244	0.336	0.246

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * p<.10, ** p<.05. Clustered standard errors by state-year are in parentheses. The sample consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration. The West excludes California. All models also control for a constant, sex, race, age at arrival (quadratic), household size, family composition, ln median neighborhood household income, and neighborhood home language concentration. Columns (3) and (6) also control for English initial language dummy, linguistic distance, and missing linguistic distance dummy.

Table 6. The Effect of the Initiatives on the ESA of Second-Generation Immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
	Arizona and Other West			Massachusetts and Other Northeast		
Treatment	-0.074** (0.013)	-0.071** (0.014)	-0.076** (0.014)	0.007 (0.007)	0.010 (0.007)	0.008 (0.008)
Post	0.066** (0.023)	0.073** (0.023)	0.063** (0.024)	0.033** (0.009)	0.039** (0.011)	0.037** (0.007)
Treatment* Post	0.116** (0.026)	0.128** (0.026)	0.116** (0.025)	0.027** (0.010)	0.024** (0.011)	0.025** (0.011)
Child Characteristics						
Nine years old	0.049** (0.014)	0.051** (0.015)	0.050** (0.014)	0.030** (0.008)	0.030** (0.008)	0.030** (0.008)
Ten years old	0.092** (0.015)	0.089** (0.015)	0.093** (0.015)	0.039** (0.009)	0.039** (0.009)	0.039** (0.009)
Household Characteristics						
Ln income	0.041** (0.006)	0.032** (0.006)	0.042** (0.006)	0.009* (0.005)	0.006 (0.005)	0.009* (0.005)
Head education	0.012** (0.003)	0.004 (0.003)	0.013** (0.003)	0.009** (0.002)	0.005 (0.003)	0.009** (0.002)
Head self- rep. ESA		0.123** (0.011)			0.063** (0.014)	
Head FE:	Country	Country	Language	Country	Country	Language
R ²	0.084	0.114	0.082	0.064	0.073	0.063

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * $p < .10$, ** $p < .05$. Clustered standard errors by state-year are in parentheses. The sample consists of second-generation immigrants, attending public school, not living in group quarters, and eight to ten years old. The West excludes California. All models also control for a constant, sex, race, household size, head years since migration (quadratic), ln median neighborhood household income, and neighborhood home language concentration. Columns (3) and (6) also control for English initial language dummy, linguistic distance, and missing linguistic distance dummy.

Columns (2) and (5) also control for the head's self-reported English speaking ability so that English speaking ability is judged by similar parents. The results indicate that the SEI initiative in Arizona is associated with a 0.150 unit decrease in parent-reported English speaking ability, which is statistically significant. The inclusion of this control variable results in a smaller average treatment effect in absolute value in Massachusetts, although it is still marginally statistically significant.

Household income and head's highest grade completed are not statistically significant in these specifications because they are highly correlated with the head's self-reported English speaking ability.

As discussed above, the sample sizes preclude the use of both source country and language fixed-effects. Columns (3) and (6) include language fixed-effects in place of source country fixed-effects. These specifications compare students of similar language backgrounds rather than countries of origin. For example, it groups together all immigrants with Spanish home languages rather than just those born in Mexico or whose household head is born in Mexico. The shift to language fixed-effects does not have an appreciable effect on the average treatment effects of SEI in either region.

Table 6 presents the results for young second-generation immigrant students that control for student, household, and neighborhood characteristics. Columns (1) and (4) present the preferred specification that controls for sex, race, age, household size, head years since migration (quadratic), household income, head grade completed, median household income, neighborhood home language concentration, and head source country fixed-effects. The results show that the Arizona SEI initiative is associated with a 0.116 unit increase in parent-reported English speaking ability, which is equivalent to 0.18 standard deviations. The results also show that the Massachusetts SEI initiative is associated with a 0.027 unit increase in parent-reported English speaking ability at a statistically significant level, which is equivalent to 0.06 standard deviations. Using the coefficient estimate for the gain in parent-reported English speaking ability between eight and nine years old, the Arizona and Massachusetts SEI initiatives are associated with developmental gains of 2.37 and 0.90 years, respectively. Since most elementary school children receive instruction that includes home language, the results indicate that for young second-generation

immigrants, the SEI initiatives are actually associated with greater parent-reported English speaking ability than Transitional Bilingual Education.

Table 7. Heterogeneous Average Treatment Effects by Home Language and Age

	(1)	(2)	(3)	(4)
	First-Generation Immigrants		Second-Generation Immigrants	
	Arizona	Massachusetts	Arizona	Massachusetts
Home language				
Spanish	-0.073 (0.096)	-0.236** (0.100)	0.121** (0.040)	0.142** (0.019)
Other languages	-0.201** (0.097)	-0.111** (0.022)	0.076** (0.018)	-0.032** (0.012)
Age				
Eight to ten years old	-0.126 (0.096)	-0.143* (0.071)		
Eleven to thirteen years old	0.106 (0.095)	-0.152** (0.059)		
Fourteen to sixteen years old	-0.413** (0.086)	-0.081** (0.038)		

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * $p < .10$, ** $p < .05$. Clustered standard errors by state-year are in parentheses. The sample in columns (1) and (2) consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration unless otherwise specified. The sample in columns (3) and (4) consists of second-generation immigrants, attending public school, not living in group quarters, and eight to ten years old unless otherwise specified. The West excludes California. All models control for a constant, treatment, post, sex, race, household size, ln median neighborhood household income, and neighborhood home language concentration. Columns (1) and (2) also control for years since migration dummy variables, age at arrival (quadratic), and child country fixed effects, and columns (3) and (4) also control for age dummy variables, head years since migration (quadratic), and head country fixed effects.

As for the other control variables, an additional year in school is positively associated with parent-reported English speaking ability, although the return to each year is lower than the return among recent-arrival first-generation immigrants. This is due to the higher initial parent-reported English speaking ability of young second-generation immigrants that comes from being born in the United States with eight to

ten years of experience in the country. Lastly, the household control variables operate in expected directions, with household income and head's education being positively associated with parent-reported English speaking ability.

I attempt to push the data further by testing for heterogeneous average treatment effects in Table 7. The idea here is to exploit the different distributions of programs by state and cohort to estimate the effectiveness of the SEI initiatives relative to specific programs. The first approach relies on the common argument that only Spanish speakers have a sufficient number of students to qualify for Transitional Bilingual Education (Christine H. Rossell 2003). The top panel presents average treatment effects from models estimated separately for students with Spanish home languages, which proxies for Transitional Bilingual Education, and for students with all other home languages, which proxies for English as a Second Language. The results indicate that for recent-arrival first-generation immigrants, Arizona Spanish speaking students are associated with a decrease in parent-reported English speaking ability but not at a statistically significant level. In contrast, non-Spanish speaking students in Arizona and all recent-arrival first-generation immigrants in Massachusetts have lower parent-reported English speaking ability with the SEI initiatives. The results for young second-generation immigrants show that Spanish speakers are the students associated with the greatest gains. One potential interpretation is that the developmental interdependence hypothesis is correct: young second-generation immigrants have sufficient English language proficiency to learn from instruction in English, but recent-arrival second-generation immigrants do not.

The bottom panel presents average treatment effects from separate regressions by age for recent-arrival first-generation immigrants. The idea here is that young children are more likely to receive Transitional Bilingual Education. The specifications also test whether the difference in average treatment effects between

recent-arrival first-generation immigrants and young second-generation immigrants are due to different age groups (eight to sixteen versus eight to ten, respectively). Unfortunately, the different average treatment effects for Arizona by age group do not lend themselves to any compelling interpretation. In contrast, the results for Massachusetts show that the oldest group experiences the smallest decrease in parent-reported English speaking ability in absolute value. However, both results for the eight to ten year old subset show that the difference in average treatment effects by nativity is not due to the difference in age groups for the two samples.

Table 8. Multinomial Logit Average Marginal Effects of the SEI Initiatives

	(1)	(2)	(3)	(4)
	Arizona and Other West			
Recent-Arrival First-Generation Immigrants				
Treatment	0.028** (0.013)	-0.021 (0.021)	-0.025** (0.012)	0.018 (0.022)
Post	-0.023* (0.014)	-0.004 (0.029)	0.049** (0.025)	-0.022 (0.034)
Treatment*Post	0.028 (0.021)	-0.021 (0.029)	0.107*** (0.028)	-0.115*** (0.031)
Young Second-Generation Immigrants				
Treatment	0.001 (0.004)	0.019** (0.009)	0.008 (0.013)	-0.028*** (0.011)
Post	-0.003 (0.005)	-0.009 (0.006)	-0.039** (0.019)	0.051*** (0.020)
Treatment*Post	-0.003 (0.011)	-0.024*** (0.007)	-0.027 (0.020)	0.054** (0.023)

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * p<.10, ** p<.05, *** p<.01. Clustered standard errors by state-year are in parentheses. The sample consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration. The West excludes California. All models also control for a constant, sex, race, age at arrival (quadratic), household size, family composition, ln median neighborhood household income, neighborhood home language concentration, and child country fixed effects. Very well includes speaks English at home.

Table 8 (Continued).

	(5)	(6)	(7)	(8)
	Massachusetts and Other Northeast			
Recent-Arrival First-Generation Immigrants				
Treatment	0.019*** (0.006)	-0.005 (0.014)	-0.016 (0.013)	0.002 (0.018)
Post	-0.008 (0.007)	0.018 (0.018)	0.002 (0.020)	-0.013 (0.020)
Treatment*Post	-0.014** (0.006)	0.046** (0.021)	0.078*** (0.021)	-0.110*** (0.019)
Young Second-Generation Immigrants				
Treatment	-0.001* (0.001)	-0.015*** (0.003)	0.031*** (0.006)	-0.015** (0.007)
Post	0.003** (0.001)	-0.008* (0.004)	-0.026*** (0.006)	0.031*** (0.004)
Treatment*Post	-0.003*** (0.000)	0.008 (0.007)	-0.024*** (0.005)	0.019*** (0.006)

Overall, the specifications show that the SEI initiatives are associated with developmental delays for recent-arrival first-generation immigrants and with developmental gains for young second-generation immigrants. The results provide suggestive evidence that the primary difference in effectiveness is between SEI and Transitional Bilingual Education, although additional cuts of the data based on home language and age group are largely unable to confirm this explanation.

V. Robustness Checks: Cardinality, Placebos, and Other Control Groups

I estimate three sets of robustness exercises in this section. The first relaxes the cardinality of parent-reported English speaking ability a multinomial logit specification, in which the dependent variable takes on each value of parent-reported English speaking ability. These models allow the SEI initiatives and control variables to have different marginal effects at each point of the parent-reported English speaking ability distribution. The average marginal effects results in Table 8 show that most of the effect of the initiatives take place between the speaks English “well” group and the

speaks English “very well” or “at home” group in both samples. In addition, in specifications not shown but available upon request, I group the speaks English “at home” with the speaks English “well” group and get similar average treatment effect estimates in the primary ordinary least squares specifications for both regions.

The second set of robustness checks test whether the initiatives affect students that should not be affected. The top panel of Table 9 presents the results of assigning a placebo to Colorado with Other West (excluding Arizona and California) as the control group. Colorado is the only other state to also consider the SEI initiative, but since it did not pass it in 2002, there should be no effect on parent-reported English speaking ability. Surprisingly, the average treatment effect in this falsification test shows that the parent-reported English speaking ability of recent-arrival first-generation immigrants actually increased by 0.244 units. The next set of falsification tests assign placebos to first-generation immigrant students with at greater years since migration in the same state since they may be fluent English proficient and learn in mainstream classrooms; the control groups are comparable students in the Other West and Other Northeast. The results show that the Arizona SEI initiative continues to have a negative effect on the parent-reported English speaking ability up through the eighth year in the United States. In contrast, the Massachusetts SEI initiative increases parent-reported English speaking ability for immigrants with three to five years since migration and has no effect for those with more than six years since migration. Overall, the falsification tests support the idea that recent-arrival first-generation immigrants who should not be affected by the SEI initiatives are indeed unaffected, although the Colorado test suggests that other unobserved state-time trends may be present in the data.

The final set of robustness checks address the presence of unobserved state-time interactions that may explain the results rather than any actual effect of the SEI

initiatives. To minimize the bias from differential trends in unobservables, I present average treatment effect estimates using alternative control groups that mirror the political conditions and program histories of the treatment groups in Table 10.

The first source of bias is that the consideration of the SEI initiatives is correlated with the selection of immigrants into the treatment group. Migration responses to local political conditions bias the average treatment effect estimator toward a beneficial effect of the SEI initiatives if less English speaking ability immigrants choose to live in surrounding control states. As discussed above, one alternative control group that has a similar immigrant policy climate is Colorado because it also considered the SEI initiative. Row (1) presents the estimates for recent-arrival first-generation immigrants that use Colorado as the control group and shows a -0.263 unit decrease in parent-reported English speaking ability due to the Arizona Proposition 203 and a -0.309 unit decrease due to the Massachusetts Question 2. For young second-generation immigrants, columns (3) and (4) show that the SEI initiative continues to be relatively more effect in Arizona but actually less effective in Massachusetts.

Older cohorts of potential ELL students in the same stated are a second control group with similar trends in unobservables. First-generation immigrants with more than six years since migration in Arizona and Massachusetts may be a suitable control group for recent-arrival first-generation immigrants because they are less likely to participate in language instruction educational programs, and the same goes for second-generation immigrants who are older than ten years old. Same-state control groups address the concern that parent assessments of English speaking ability are responding to the outcome of the election rather than a change in the child's English language proficiency. However, a concern is that the SEI initiatives may also affect the student achievement of non-ELL students due to changes in peer groups and

district financial resources (Nora Gordon and Caroline Hoxby 2002). With this caveat in mind, in Row (2), I use the same-state first-generation immigrants with six to eight years since migration as the control group and find a similar negative effect of the Arizona SEI initiative, although it is not statistically significant. In contrast, I find that the average treatment effect of the Massachusetts SEI initiative is -0.254 units. In Row (3), I repeat this process for second-generation immigrants using eleven to thirteen years old children as the control group, and again there is a benefit of the SEI initiative in Arizona but not in Massachusetts.

The third alternative control group uses state English only laws to identify states with a similar immigrant policy climate. States with English only laws may be similar to Arizona and Massachusetts because they encourage the use of English over home languages. Row (4) uses the set of English only states excluding California, Arizona, and Massachusetts as the control group. The average treatment effect estimates are negative and statistically significant for both Arizona and Massachusetts for recent-arrival first-generation immigrants. And again, the SEI initiatives are associated with a developmental gain for young second-generation immigrants in Arizona but not in Massachusetts.

The second set of alternative control groups consists of states with similar program distributions before the SEI initiatives. Anecdotal evidence suggests that districts that use Transitional Bilingual Education are shifting instruction away from home languages and toward English to meet the assessment requirements of NCLB (Mary A. Zehr 2007). To check whether the negative effect of the SEI initiatives among recent-arrival first-generation immigrants is instead attributable to responses to NCLB, I use the five states that currently require Transitional Bilingual Education — Illinois, New Jersey, New Mexico, New York, and Texas — as an alternative control group for Massachusetts. The average treatment effect estimate for Massachusetts is -

0.111 units for recent-arrival first-generation immigrants. For young second-generation immigrants, the results show no effect of the Massachusetts SEI initiative.

Table 9. State and Cohort Falsification Tests

	(1)	(2)	(3)
	Colorado and Other West	Arizona and Other West	Massachusetts and Other Northeast
Recent-Arrival First-Generation Immigrants			
Zero to two years since migration	0.244** (0.077)		
Three to five years since migration		-0.113** (0.054)	0.048** (0.016)
Six to eight years since migration		-0.070** (0.023)	0.017 (0.019)
Nine or more years since migration		0.005 (0.015)	0.006 (0.013)
Young Second-Generation Immigrants			
Eight to ten years old	-0.130** (0.014)		
Eleven to thirteen years old		0.037** (0.016)	-0.008 (0.009)
Fourteen to sixteen years old		-0.030* (0.015)	0.011 (0.011)

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * $p < .10$, ** $p < .05$. Clustered standard errors by state-year are in parentheses. The sample in the top panel consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration unless otherwise specified. The sample in the bottom panel consists of second-generation immigrants, attending public school, not living in group quarters, and eight to ten years old unless otherwise specified. The West excludes California. All models control for a constant, treatment, post, sex, race, household size, ln median neighborhood household income, and neighborhood home language concentration. The top panel also controls for years since migration dummy variables, age at arrival (quadratic), and child country fixed effects, and the bottom panel also controls for age dummy variables, head years since migration (quadratic), and head country fixed effects.

Table 10. Average Treatment Effects Using Alternative Control Groups

	(1)	(2)	(3)	(4)
	Recent-Arrival First-Generation Immigrants		Young Second-Generation Immigrants	
Alternative Control Group	Arizona	Massachusetts	Arizona	Massachusetts
Colorado	-0.263** (0.019)	-0.309** (0.030)	0.094** (0.015)	-0.040** (0.016)
Same state, six to eight YSM	-0.110 (0.087)	-0.254** (0.076)		
Same state, 11- 13 years old			0.119** (0.040)	0.025 (0.041)
English only law states	-0.165** (0.029)	-0.251** (0.030)	0.096** (0.019)	-0.046* (0.024)
States without TBE laws	-0.110** (0.028)		0.071** (0.016)	
States with TBE laws		-0.111** (0.035)		-0.022 (0.021)
States with similar programs	-0.126** (0.061)	-0.229** (0.059)	0.134** (0.025)	0.005 (0.027)

Sources: 2000 Census and 2005, 2006, 2007 American Community Survey.

Notes: * $p < .10$, ** $p < .05$. Clustered standard errors by state-year are in parentheses. The sample consists of first-generation immigrants, attending public school, not living in group quarters, eight to sixteen years old, and with zero to two years since migration. The West excludes California. All models also control for a constant, sex, race, age at arrival (quadratic), household size, family composition, ln median neighborhood household income, and neighborhood home language concentration. Columns (1) and (2) also control for years since migration dummy variables (as applicable), age at arrival (quadratic), and child country fixed effects, and columns (3) and (4) also control for age dummy variables (as applicable), head years since migration (quadratic) and head country fixed effects. Row (2) also controls for years since migration (quadratic), and row (3) also controls for age (quadratic).

The last alternative control group uses states with similar program distributions from Kindler's (2002) analysis of state survey data. I select the nearest five states above and below Arizona and Massachusetts in the share of ELL students that received instruction that incorporates home languages as the alternative control groups. The average treatment effect estimates are qualitatively similar to those of the

primary specification for both samples: recent-arrival first-generation immigrants have higher parent-reported English speaking ability using previous programs in both Arizona and Massachusetts, and young second-generation immigrants in Arizona have higher parent-reported English speaking ability with the SEI initiatives.

Thus, the robustness checks confirm that the Arizona and Massachusetts SEI initiatives decrease the parent-reported English speaking ability of recent-arrival first-generation immigrants and increase the parent-reported English speaking ability of young second-generation immigrants in Arizona. The results raise some doubt that the Massachusetts initiative actually had a positive effect among young second-generation immigrants and suggests instead that it is equally effective as previous programs.

VI. Conclusion and Policy Implications

Over 6.41 million people in California, Arizona, Massachusetts, and Colorado voted in support of initiatives that require K–12 public school districts to provide ELL students with SEI for a period not normally to exceed one year. The initiatives were passed in California, Arizona, and Massachusetts and were intended to accelerate the development of English language proficiency relative to previous programs, such as Transitional Bilingual Education and English as a Second Language. As of 2005, nearly one million ELL students are subject to the SEI initiatives. What does the data say about the relative effectiveness of Arizona Proposition 203 and Massachusetts Question 2?

This paper presents evidence that Arizona Proposition 203 and Massachusetts Question 2 are less effective than previous programs in developing the English language proficiency of recent-arrival first-generation immigrants. The average treatment effect estimates correspond to developmental delays of an additional 0.43 to 0.73 years of school. These results are consistent with Gordon and Hoxby's (2002) evidence that California Proposition 227 decrease the reading and language test scores

of ELL students. However, I also show that the relative effectiveness of the SEI initiatives on young second-generation immigrants is positive. To the best of my knowledge, this paper is the first to show heterogeneous average treatment effects by nativity, where the SEI initiative is associated with a developmental gain of 0.90 to 2.37 years of school. These results imply that a one size fits all instructional policy does not serve ELL students well. There are important differences between ELL students that lead to different optimal programs for different groups of ELL students.

The different in average treatment effects beg the question of why recent-arrival first-generation immigrants react differently to the SEI initiatives than young second-generation immigrants. There are at least two potential explanations that suggest future avenues of research. The first is that the higher initial English language proficiency of young second-generation immigrants makes them more receptive to instruction in English, even though they are classified as ELL. Indeed, there is experimental evidence that second-generation immigrants perform better on tests in English than in home languages (Richard Akresh and Ilana Redstone Akresh 2008), which would explain the advantage of the SEI initiatives over Transitional Bilingual Education. A second explanation is that young second-generation immigrants attend higher quality schools than recent-arrival first-generation immigrants. Parents of young second-generation immigrants have greater human capital as measured by highest grade completed and self-reported English speaking proficiency than recent-arrival first-generation immigrants, which may lead them to live in wealthier neighborhoods with better schools.

My results have important implications for ELL students and stakeholders in K–12 public education. For ELL students, the importance of the relative effectiveness of the SEI initiatives is obvious given that English language proficiency is positively associated with schooling, occupational choice, and wages. The results are also

important to all stakeholders in K–12 public education due to the accountability of provisions of NCLB. The designation of ELL students as a special subgroup in Title I increases their relative importance as schools aim for adequate yearly progress, and Title III imposes annual measurable achievement objectives for their English language proficiency. The accountability measures for the long-run failure to meet these objectives include reduced federal funds and the dismissal of administrators and teachers, which would affect the instruction for all students regardless of language background. Moreover, the failure to develop English language proficiency, even in the short-run, may affect the community at large by depressing local housing prices if it decreases public measures of school quality (Sandra E. Black 1999; David N. Figlio and Maurice E. Lucas 2004).

Finally, the results also relate to whether the initiative process is a good way to set public policy. On the one hand, initiatives allow voters to overcome principal-agent problems between the electorate and policymakers. District administrators, teachers, and state legislators are often not accountable for using relatively ineffective programs that serve their own interests, and the initiative process allows the electorate to compel agents to switch to more effective programs. On the other hand, it is unlikely that the average voter has an expert opinion on how to rapidly develop proficiency in a second language, even if the language in question happens to be the voter's native tongue. When expert information is unavailable to the electorate, legislative bodies and non-accountable policymakers (i.e. tenured teachers) often adopt better policies than citizens in a direct democracy (Eric Maskin and Jean Tirole 2004). This paper demonstrates that after half a million dollars in Arizona and one million dollars in Massachusetts were spent on campaigns to inform voters, the electorate adopted initiatives that are less effective for the majority of ELL students than the programs chosen by districts, teachers, and state legislators. However, it did benefit young

second-generation immigrants who make up a substantial portion of the ELL student population. At least in the case studies of Arizona Proposition 203 and Massachusetts Question 2, the disadvantages of setting policy with initiatives appear to outweigh the advantages.

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CHAPTER 2

MEASUREMENT ERROR, MISSPECIFICATION, AND THE RETURN TO FOREIGN EDUCATION

I. Introduction

Estimates using the U.S. Census and the American Community Survey show that between 1970 and 2005, the immigrant share of the labor force tripled, from 5.8 percent to 16.8 percent.⁹ Immigrant workers are an economically vulnerable population because their wages are lower than those of natives with similar observables. A primary suspect for explaining the immigrant-native wage gap is the return to foreign education (Gilles Grenier 1984), which may be less than the return to domestic education among natives if human capital is not completely portable or if the quality of education differs between countries. Thus, an accurate estimate of the return to foreign education may help us understand the wage structure of this increasingly important segment of the labor market. It also has implications for policymakers who argue that admitting prospective immigrants with greater human capital decreases the net fiscal burden of immigration on taxpayers (Jonathan Weisman 2007).

Previous studies of the return to foreign education in the United States yield estimates between 4.2 and 5.9 percent (Julian R. Betts and Magnus Lofstrom 2000; Barry R. Chiswick 1978; Robert F. Schoeni 1997), which is less than the return to

⁹ The data are from the one percent Form 1 State sample of the 1970 U.S. Census and the one percent sample of the 2005 American Community Survey (Steven Ruggles, et al. 2008). The sample consists of men and women between ages 16 and 64, with at least one year of potential work experience, and who worked last year.

domestic education among natives (for a review, see David Card 1999). However, these studies are subject to two econometric problems. The first is that foreign education is measured with error – data limitations in the U.S. Census have led scholars to calculate foreign education with a piecewise function of total education and age at arrival. With a school starting age of six years and a maximum of 18 years of total education, the piecewise function implies that all immigrants whose age at arrival is at least 25 years old completed their total education abroad. This assumption seems implausible given that older immigrants are more likely to attend school in the United States than natives of similar age (Julian R. Betts and Magnus Lofstrom 2000). If domestic education is more valuable than foreign education, this form measurement error will lead to upward bias in the return to foreign education.

The second problem is that previous studies include domestic education as an endogenous control variable in their econometric specifications. Given the interest in the overall causal effect of foreign education on wages, domestic education is endogenous because it is determined by foreign education – part of the return to foreign education operates through its effect on investment in domestic education. The direction of the misspecification bias depends on whether domestic education is positively or negatively correlated with foreign education. On the one hand, highly-educated immigrants may be more likely to have attended school in the United States if foreign education proxies for income or savings at the time of migration. On the other hand, less-educated immigrants may be more likely to have attended school in the United States if foreign education proxies for the opportunity cost of school enrollment. Unfortunately, the literature has yet to reach on consensus on the sign of this correlation, so the bias from controlling for domestic education, and thus the overall bias in previous studies, is ambiguous.

In this paper, I make two contributions to the literature using longitudinally-

linked data between the 1995, 1999, and 2004 October Supplements of the Current Population Survey (CPS) and the nearest Outgoing Rotation Groups in the CPS basic monthly survey. First, I study the measurement error in previous studies due to calculating foreign education with the piecewise function of total education and age at arrival. The unique data in the October Supplements on schooling earned in the United States allows me to directly calculate foreign education as the difference between total education and domestic education. Second, I analyze the bias from including domestic education as an endogenous control variable. In particular, the supplemental data allows me to estimate the relationship between foreign education and domestic education.

Using the standard specification in previous studies, the CPS data show a 5.8 percent return to foreign education among immigrant men whose age at arrival is at least 25 years. However, the data reveal substantial measurement error in foreign education, with 25 percent of immigrants having attended school in the United States, instead of zero percent as implied by the piecewise function of total education and age at arrival. Correcting for measurement error leads to a lower return of 5.4 percent, which is consistent with the prior that domestic education is more valuable than foreign education. Excluding domestic education as an endogenous control variable leads to an even lower return to foreign education of 3.3 percent. The upward bias from over-controlling the specification is due to the negative correlation between foreign education and domestic education – an additional year of foreign education is associated with 0.32 less years of domestic education. These two corrections result in an estimated return to foreign education that is considerably lower than those from previous studies, which is primarily due to omitting domestic education as an endogenous control variable. The results are robust to (1) quantile regressions that reduce the bias from the top-coding of wages, (2) alternative samples based on age at

arrival and potential foreign work experience, (3) corrections for panel attrition, and (4) corrections for classical measurement error.

The remainder of this paper is organized as follows. Section II discusses previous studies, and section III describes the CPS data and research design used in this paper. Section IV confirms the endogeneity of domestic education, section V decomposes the bias in previous estimates of the return to foreign education, and section VI presents the robustness exercises. Lastly, section VII discusses the implications for labor economics and immigration policy.

II. Econometric Approaches to Estimating the Return to Foreign Education

A. The Thought Experiment

The research objective is to measure the overall value of foreign education among immigrants in the United States. The corresponding thought experiment is to randomly assign foreign education to immigrants at the time of migration and then estimate its effect on wages. With this setup, endogenous post-migration investment in human capital should be excluded from the econometric specification because it is part of the return to foreign education. This argument is same as the one against controlling for occupation when studying the gender pay gap (Francine D. Blau and Lawrence M. Kahn 2006) and the one against controlling for graduate degree when studying the return to college quality (Dominic J. Brewer, Eric R. Eide and Ronald G. Ehrenberg 1999; Liang Zhang 2005).

B. Decomposing the Bias in Previous Studies

The standard econometric approach is to run a regression of wages on foreign education, controlling for exogenous characteristics at the time of migration. To fix ideas, suppose that the true model is Equation (2) and that the error term and foreign education are uncorrelated. Throughout this section, I abstract from all other control variables. Using the ordinary least squares estimator, the probability limit of the

coefficient on foreign education is shown in Equation (3) – the model is correctly specified, and the estimator is unbiased. The Technical Appendix presents the derivations of all equations used in this paper.

$$Y = \alpha_0 + E_f \alpha_1 + \varepsilon \quad (2)$$

$$p \lim \hat{\alpha}_1 = \alpha_1 \quad (3)$$

In practice, however, scholars are subject to a data limitation that generates a specific form of measurement error in foreign education. Workhorse datasets such as the U.S. Census, the American Community Survey, and the CPS basic monthly survey do not contain data on education by country of origin. Beginning with Chiswick (1978), the standard approach is to calculate foreign education (E_f) using total education (T) and age at arrival (A) with the piecewise function in Equation (4).¹⁰ For example, an immigrant who arrived at age 20 years with 12 years of total education earned all 12 years abroad; an immigrant who arrived at age 20 years with 16 years of total education earned only 14 years abroad and two years of schooling in the United States.

$$E_f = \begin{cases} T & \text{if } A \geq T + 6 \\ A - 6 & \text{if } 6 < A < T + 6 \\ 0 & \text{if } A \leq 6 \end{cases} \quad (4)$$

To simplify the analysis, and to ensure that migration does not interrupt

¹⁰ A second source of measurement error is caused by the interval reporting of year of arrival, which generates attenuation bias in the ordinary least squares estimator. More recent studies use datasets that report year of arrival in years rather than in intervals (Randall K. Q. Akee and Mutlu Yuksel 2008; Rachel M. Friedberg 2000). A third source of measurement error is due to multiple trip-taking to the United States (Ilana Redstone and Douglas S. Massey 2004).

schooling and thus censor foreign education, I restrict my attention to the top part of the function. Measurement error in this range is due to the misclassification of all domestic education as foreign education since foreign education is equal to total education. This error can be thought of as a linear restriction that forces the returns to foreign education and domestic education to be equal. To see its effect on the return to foreign education, assume that the econometric specification is Equation (5) and Equation (6) and that domestic education is exogenous. Let α_2 be the coefficient on domestic education in the unrestricted regression with foreign education and domestic education as independent variables. As seen in Equation (7), if domestic education is more valuable in the domestic labor market than foreign education ($\alpha_2 > \alpha_1$), then the measurement error generates upward bias in the return to foreign education. If domestic education is less valuable than foreign education ($\alpha_2 < \alpha_1$), then the measurement error leads to downward bias in the return to foreign education. Because most studies present evidence that the return to foreign education is less than the return to domestic education (Bernt Bratsberg and James F. Jr Ragan 2002; Rachel M. Friedberg 2000; Robert F. Schoeni 1997; James B. Stewart and Thomas Hyclak 1984), the piecewise function of total education and age at arrival leads to upward bias in the return to foreign education.

$$Y = \delta_0 + \delta_1 E_f^* + \nu \quad (5)$$

$$E_f^* = T = E_f + E_d \quad (6)$$

$$p \lim \hat{\delta}_1 = \alpha_1 + (\alpha_2 - \alpha_1) \frac{\text{var}(E_d)}{\text{var}(E_f) + \text{var}(E_d)} \quad (7)$$

A second econometric problem is model misspecification. While the research objective is to determine the overall causal effect of foreign education on wages in the domestic labor market, scholars instead estimate regressions of wages on foreign education that control for domestic education (Julian R. Betts and Magnus Lofstrom

2000; Bernt Bratsberg and James F. Jr Ragan 2002; Rachel M. Friedberg 2000; Robert F. Schoeni 1997). The inclusion of domestic education as a control variable leads to bias in the return to foreign education if it is endogenous. Indeed, Duleep and Regets (1999) argue that it is exactly the difference in the returns to foreign education and domestic education that causes post-migration educational investment.

To understand this potential problem, suppose that Equation (2) is the true model, but that instead we estimate Equation (8). Now, assume that foreign education is not subject to measurement error caused by the piecewise function in Equation (4). In this case, the coefficient on foreign education has the probability limit shown in Equation (10), where the bias depends on the relationship between domestic education and foreign education (τ_1) and the coefficient from the regression of the wage residuals (ε) on the domestic education investment residuals (ν).

$$Y = \lambda_0 + E_f \lambda_1 + E_d \lambda_2 + \psi \quad (8)$$

$$E_d = \tau_0 + \tau_1 E_f + \nu \quad (9)$$

$$p \lim \hat{\lambda}_1 = \alpha_1 - \tau_1 \frac{\text{cov}(\varepsilon, \nu)}{\text{var}(\nu)} \quad (10)$$

It is likely that the rightmost term in Equation (10) is positive because unobservables that lead to greater investment in human capital are also likely to lead to greater wages. Thus, if less-skilled immigrants have greater investment in domestic education ($\tau_1 < 0$), controlling for domestic education biases the return to foreign education upward. On the other hand, if more-skilled immigrants have greater investment in domestic education ($\tau_1 > 0$), controlling for domestic education biases the return to foreign education downward. There is substantial disagreement over the empirical relationship between foreign and domestic education, with some arguing that it is negative (George J. Borjas 1982; Aliya Hashmi Khan 1997) and others that it is positive (Ilana Redstone Akresh 2007; Barry R. Chiswick and Paul W. Miller 1994; Deborah Cobb-Clark, Marie D. Connolly and Christopher Worswick 2005), so the

effect of misspecification bias, and thus the total bias in previous studies, is of ambiguous direction.

III. Data and Research Design

A. Data Construction

To estimate the corrected return to foreign education, I use the Current Population Survey, which is a monthly survey of households in the United States. The basic monthly survey includes information on total education, demographic characteristics, and labor supply for household members. Each household is interviewed for four months, on break for eight months, and then re-interviewed for four months. Households units have staggered months of entry, so that each month, one-eighth of the sample is in the first month in sample, one-eighth of the sample is in the second month in sample, and so on, up to one-eighth of the sample in the eighth and last month in sample.

The CPS is useful for estimating the return to foreign education because immigrants report schooling earned in the United States in the 1995, 1999, and 2004 October Supplements. I calculate foreign education as the difference between total education and domestic education rather than compute it with the piecewise function of total education and age at arrival. Total education is reported in intervals, so I set education as 0 for less than one year, 2.5 for one to four years, 5.5 for five or six years, 7.5 for seven or eight years, 9 for 9 years, 10 for 10 years, 11 for 11 years, 12 for 12 years or high school graduate, 13 for some college, 14 for associate degree, 16 for bachelor's degree, and 18 for master's degree, professional school degree, or doctorate degree (David A. Jaeger 1997). In contrast, domestic education (for immigrants) is reported as years of school attended in the United States. I cap domestic education at

total education and drop a small number of observations with missing domestic education or with negative foreign education.¹¹

The wage data in the basic monthly survey is only available for households in the fourth and eighth months in sample, which are called the Outgoing Rotation Groups. I use the longitudinal feature of the CPS to match household members between the October Supplements and the nearest Outgoing Rotation Groups in the October, November, December, and January basic monthly surveys.¹² For example, a household in October 1995 in its second month in sample is interviewed again in December 1995 when it is in its fourth month in sample. Because the CPS follows households rather than people, longitudinal matches may incorrectly link two different people together. I follow Madrian and Lefgren (2000) and condition matches on age within two years, sex, and race and ethnicity, as well as on country of birth, mother's and father's countries of birth, employment status, and sector of work for a match rate of 79.5 percent.¹³ The sample in this paper is based on the October Supplements with its population weights. Panel attrition in the form of non-matches between the October Supplements and the basic monthly surveys causes missing data on wages,

¹¹ Of the eligible 4,356 immigrants in the October Supplements, 112 are dropped for missing domestic education, 17 have domestic education capped at total education, and 48 are dropped because their foreign education is less than zero, for a total of 4,196 immigrants eligible to be matched to the basic monthly surveys.

¹² I do not link respondents to one year forward or backward since the match rates are lower and school enrollment during the time elapsed will lead to measurement error in domestic education.

¹³ Non-matches are also caused by having trimmed hourly wages.

and I drop these respondents from the sample.

I deflate the wage data into 2000 dollars and follow Lemieux (2006) by using hourly wage if it is available. For all other workers, I use CPS calculated weekly wage divided by usual hours worked last week. CPS weekly wages are subject to top-codes that vary over time, so I impose the minimum common top code across all years (\$2021.57 in 2000 dollars) on the data and replace top-coded wages with 1.4 times the new top code. As for usual hours, respondents may report “hours vary,” so I impute their labor supply using regressions of usual hours on actual hours, number of jobs, and part-time labor supply by sex, basic monthly survey month-year, and nativity type.

The sample consists of immigrant and native men to abstract away from selective labor force participation among women. I define immigrants as foreign-born and natives as United States-born with both parents born in the United States. I also restrict the sample to workers in the wage and salary sector, and I exclude respondents living in group quarters and immigrants with missing years since migration.

The final sample restriction is due to a migration timing problem that is rarely made explicit. Immigrants with interrupted schooling, such as people who migrate as children and people who come on F1 student visas, have censored values of foreign education. It is difficult to know how much foreign education they would have gotten if they stayed in their countries of birth. Thus, their domestic education is the sum of schooling they would have earned in the source country and schooling they would have earned as a result of their economic condition at migration. There are two general approaches to dealing with this problem: (1) restricting the sample to immigrants with sufficient time to complete their foreign education using age at arrival (Julian R. Betts and Magnus Lofstrom 2000; Aliya Hashmi Khan 1997), and (2) using data on immigrant visa type (Barry R. Chiswick and Paul W. Miller 1994; Deborah Cobb-Clark, Marie D. Connolly and Christopher Worswick 2005). Due to

the data limitations of the CPS, I restrict the sample to first-generation immigrants who were at least 25 years old when they arrived. Conveniently, this restriction means that according to the piecewise function in Equation (7), foreign education is equal to total education.

Table 11. Sample Means and Standard Deviations

	(1)	(2)		(3)	(4)
	Immigrants	Natives		Immigrants	Natives
ln(Hourly wage)	2.552 (0.634)	2.768 (0.555)	Potential total work experience	25.686 (10.815)	21.802 (10.208)
Hourly wage	15.974 (12.379)	18.646 (11.632)	Potential foreign work experience	16.223 (8.698)	–
Labor hours per week	41.358 (7.826)	43.108 (8.398)	Years since migration	10.441 (8.240)	–
Total education	12.329 (4.626)	13.675 (2.326)	Arrived 1900– 1969	0.025 (0.155)	–
Foreign education	11.352 (4.668)	–	Arrived 1970– 1989	0.391 (0.488)	–
Domestic education>0	0.250 (0.433)	1.000 (0.016)	Arrived 1990– 2009	0.584 (0.493)	–
Domestic education	0.977 (2.536)	13.675 (2.326)	Citizen	0.300 (0.458)	–
White	0.231 (0.422)	0.844 (0.363)	Married	0.708 (0.455)	0.694 (0.461)
Black	0.086 (0.280)	0.110 (0.313)	Number of children	0.871 (1.133)	0.801 (1.096)
Hispanic	0.434 (0.496)	0.032 (0.177)	Number of adults	2.612 (1.194)	2.152 (0.757)
Asian or Pacific Islander	0.244 (0.429)	0.005 (0.068)	Metropolitan	0.955 (0.208)	0.800 (0.400)
Other race	0.006 (0.074)	0.009 (0.092)	N	3337	50981

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old.

Table 11 presents the sample means and standard deviations for immigrants and natives. The average hourly wage for immigrants is \$2.7 less than that of natives, although both groups have similar usual hours worked per week. The average total education among immigrants is 12.3 years, which is all of foreign origin according the piecewise function of total education and age at arrival. The data in the October Supplements rejects this assumption, with 25 percent of immigrants having attended school in the United States. The new measure shows that on average, immigrants only have 11.4 years of foreign education, with an additional 1.0 years of education earned after migration.

The descriptive statistics also show that immigrants are more likely to be Hispanic or Asian and Pacific Islander. For immigrants, I focus on potential foreign work experience rather than age because it provides a more straightforward interpretation of years since migration – controlling for potential foreign work experience implies that an additional year since migration is associated with an additional year in age, while controlling for age implies that an additional year since migration is associated with a one year decrease in the age at arrival.¹⁴ Immigrants have more potential total work experience than natives, which is partly due to their lower total education and partly due to their younger age. Most of the immigrants in the sample arrived between 1970 and 2000, a period in which cohort differences have been shown to affect immigrant wages (George J. Borjas 1985; George J. Borjas 1995). As for family composition, immigrants are equally likely to be married but are more likely to live in larger households than natives. Immigrants are also more likely

¹⁴ Age is equal to six plus foreign education plus potential foreign work experience plus years since migration.

to live in metropolitan areas. The distribution of their countries of birth is presented in Table 12. I reassign immigrants from countries with less than 20 observations to the appropriate geographical residual code in the CPS.¹⁵ As expected, the largest group of immigrants in the sample consists of people born in Mexico, followed by people born in the Philippines, India, China, and then El Salvador. There are stark differences in the amount of foreign education by country, ranging from a low of 7.5 years of foreign education for Guatemalan immigrants to a high of 15.3 years of foreign education for Russian immigrants.

Table 12. Immigrant Countries of Birth

Country	N	ln(Hourly wage)	Hourly wage	Foreign education	Domestic education>0	Domestic education
Mexico	698	2.185	9.846	7.925	0.142	0.422
Philippines	205	2.667	17.154	13.367	0.193	0.966
India	181	2.993	23.664	14.746	0.355	1.611
China	170	2.796	21.121	13.300	0.450	1.747
El Salvador	95	2.259	10.684	7.621	0.160	0.358
Canada	88	3.181	27.716	13.858	0.246	1.557
Cuba	88	2.348	12.245	11.441	0.112	0.311
Dom. Rep.	81	2.275	11.702	9.474	0.157	0.632
Vietnam	80	2.551	14.760	11.417	0.382	1.075
Russia	72	2.806	19.474	15.302	0.184	0.643
Poland	68	2.595	14.806	13.063	0.132	0.508
England	58	3.324	32.397	14.338	0.291	1.747
Haiti	57	2.313	11.509	10.747	0.273	1.027
South Korea	56	2.807	19.872	14.135	0.285	1.455
Guatemala	55	2.166	9.426	7.547	0.068	0.113
Colombia	49	2.544	14.405	11.934	0.308	1.063
Peru	46	2.413	12.758	12.402	0.329	1.286

¹⁵ I also combine the residual North America with Elsewhere (includes country not known) because it has less than 20 observations.

Table 12 (Continued).

Puerto Rico	44	2.452	13.613	9.211	0.266	1.892
Jamaica	42	2.522	13.347	10.698	0.263	1.058
Taiwan	42	3.070	26.096	14.285	0.538	1.721
Honduras	37	2.280	10.773	9.312	0.108	0.477
Japan	36	3.496	36.942	15.135	0.177	0.590
Germany	35	3.232	29.267	15.222	0.276	1.005
Guyana	33	2.480	13.169	10.312	0.329	1.485
Ecuador	30	2.193	9.927	10.615	0.268	0.721
Brazil	27	2.536	15.450	12.083	0.206	0.934
Ukraine	27	2.616	16.218	13.645	0.259	0.588
Iran	25	2.961	23.059	14.200	0.446	1.601
France	22	3.076	26.122	16.557	0.097	0.350
Romania	20	2.519	13.505	12.963	0.496	1.898
Nicaragua	20	2.408	12.492	10.812	0.208	1.300
Pakistan	20	2.644	16.290	13.576	0.405	1.707
Asia	134	2.619	16.487	12.988	0.341	1.092
Caribbean	31	2.560	15.714	10.779	0.405	1.806
C. America	31	2.380	11.934	10.171	0.150	0.909
Europe	148	2.884	23.005	13.095	0.258	0.919
Middle East	35	2.892	22.308	12.956	0.417	1.886
Other Africa	114	2.682	18.663	12.888	0.488	2.078
Pacific Is.	38	2.793	22.106	13.215	0.388	1.383
S. America	50	2.736	19.019	12.453	0.315	1.364
Elsewhere	149	2.592	16.079	12.926	0.267	1.165

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old.

Figure 3 shows three non-parametric relationships between education and natural log hourly wages with local linear regressions. The solid line shows a positive association between domestic education and wages among natives, with a slightly greater slope among people with at least 11 or 12 years of school. As for immigrants,

there are two important takeaways from the figure. The first is that the slope of the dashed line for immigrant foreign education is flatter than the solid line for native domestic education, which implies a lower return to foreign education among immigrants than the return to domestic education among natives. The second is that the dashed line for foreign education is almost always above the dotted line for immigrant total education. Thus, for a given level of education, immigrants with only foreign education earn more than immigrants with some foreign education and some domestic education.

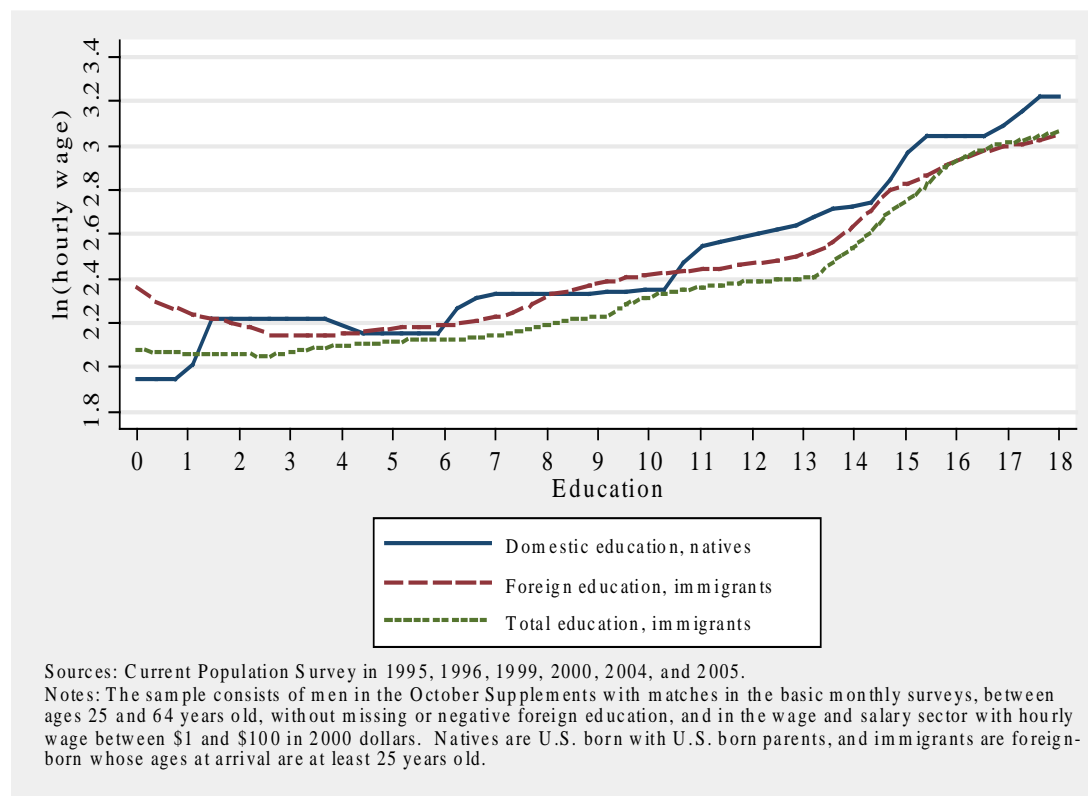


Figure 3. Local Linear Regressions of Wages on Education

Figure 4 presents the relationship between foreign education and domestic education among immigrants. The solid line for the fraction that attended school in the United States as a function of foreign education does not suggest any meaningful trend. It is unclear why there is a local maximum at 10 years of school, but one

possible explanation is that immigrants misunderstand the question about domestic education as one about total education. The dashed line shows unconditional average years of domestic education as the dependent variable, which is mostly downward sloping – immigrants with less foreign education have more domestic education than immigrants with more foreign education. Lastly, the dotted line shows the average years of domestic education conditional on having any domestic education as the dependent variable, and again, immigrants with less foreign education make greater investments in domestic education.

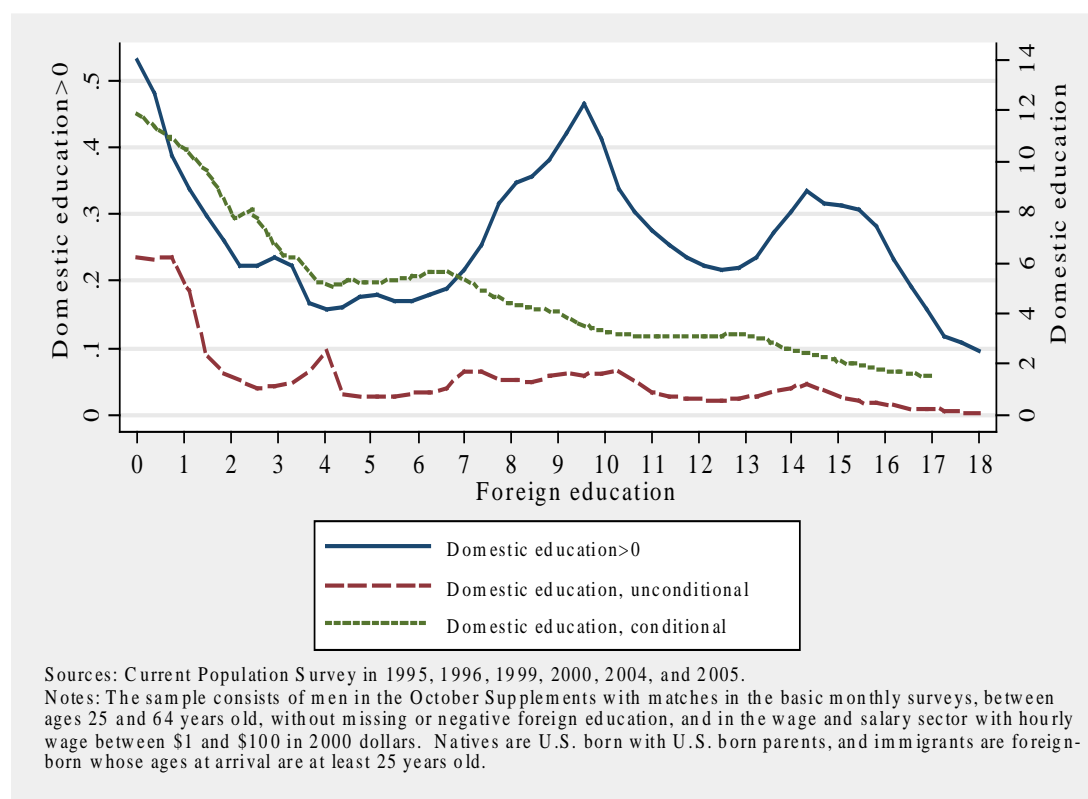


Figure 4. Local Linear Regressions of Domestic Education on Foreign Education

These figures suggest that the measurement error and model misspecification in previous studies both lead to downward bias in the estimated return to foreign education. However, since they do not control for other important variables that vary among immigrants, I turn next to parametric analyses to hold these factors constant.

B. *Research Design*

As a preliminary analysis, I first estimate the relationship between domestic and foreign education among immigrants using Equation (11). The dependent variable is either (1) having any domestic education or (2) domestic education, and I use logistic and ordinary least squares estimators, respectively. In the latter case, the parameter γ_1 corresponds to τ_1 in Equation (9), which allows me to sign the bias from over-controlling the econometric specification with domestic education. In the baseline specification, I control for a vector of exogenous variables, \mathbf{X} , which consists of potential foreign work experience, years since migration, and cohort fixed effects for immigrants and potential domestic work experience for natives. I also use specifications that add a vector of potentially endogenous variables in \mathbf{Z}_1 , which consists of citizenship (only for immigrants), married, number of children in the household, number of adults in the household, residence in a metropolitan area, and Census division fixed effects. These variables are potentially endogenous because they take place at the time of the survey rather than at the time of migration. The error term includes basic monthly survey fixed effects (October 1995 to January 2004) and country of birth fixed effects (only for immigrants).

$$f(E_d) = \Lambda(\gamma_0 + \gamma_1 E_f + \gamma_2 \mathbf{X} + \gamma_3 \mathbf{Z}_1 + \nu) \quad (11)$$

I use similarly specified wage models as shown in Equation (12), one with the exogenous controls only and one that adds the potentially endogenous controls. The only difference is that the wage equations also control for part-time employment and union coverage as potentially endogenous control variables in \mathbf{Z}_2 , which are likely to affect wages but irrelevant for domestic educational investment. For comparison, I also estimate the wage specifications separately for natives, with a focus on the return to domestic education and controlling for potential domestic work experience. In addition to estimating the wage models for immigrants, I also estimate them separately

for natives for comparison with the focus on the return to domestic education and control for potential domestic work experience. Note that the basic monthly survey fixed effects controls for common labor market conditions across all workers in each group. As extensions, I also experiment with non-linear specifications of foreign education given the graphical evidence presented in Figure 3.

$$\ln w = \beta_0 + \beta_1 E_f + \beta_2 \mathbf{X} + \beta_3 \mathbf{Z}_1 + \beta_4 \mathbf{Z}_2 + \mu \quad (12)$$

One major concern with this identification strategy is the potential bias from selective emigration. Ideally, the sample consists of all immigrants who chose to migrate. Instead, the data only contains information on immigrants who are still in the United States at the time of the survey. Previous studies document substantial emigration that varies by demographic characteristics and country of birth (e.g. Jennifer Van Hook, et al. 2006). Indeed, estimates from longitudinal Social Security Administration data show lower assimilation rates than those based on cross-sectional data (Darren Lubotsky 2007). To the extent that emigration is negatively correlated with foreign education, previous estimates of the return to foreign education, and the ones in this paper, are upper bounds on the true return to foreign education.

IV. The Endogeneity of Domestic Education

Table 13 presents the results of the domestic education investment models. Column (1) shows the average marginal effects from a logistic regression with having attended school in the United States as the dependent variable. Each year of foreign education is associated with a 2.9 percentage point decrease in the probability of having any domestic education. This result is similar to Khan's (1997) analysis of the 1976 Survey of Income and Education, but the advantages of the 1995, 1999, and 2004 October Supplements are that they are much more recent and that allow me to include cohort fixed effects. Immigrants with greater potential foreign work experience are less likely to have attended school in the United States, which may

reflect greater opportunity costs of school enrollment.

Table 13. Immigrant Investment in Domestic Education

	(1)	(2)	(3)	(4)
	Domestic education>0 Logistic average marginal effects		Domestic education Ordinary least squares	
Foreign education	-0.029** (0.002)	-0.030** (0.002)	-0.316** (0.024)	-0.327** (0.024)
Potential foreign work experience	-0.006** (0.001)	-0.005** (0.001)	-0.027** (0.006)	-0.028** (0.006)
Years since migration	0.003 (0.002)	0.000 (0.002)	0.007 (0.010)	-0.013 (0.011)
Arrived 1900–1969	0.006 (0.068)	0.034 (0.072)	0.061 (0.441)	0.176 (0.441)
Arrived 1970–1989	-0.010 (0.029)	-0.006 (0.029)	-0.061 (0.155)	0.001 (0.155)
Citizen		0.099** (0.022)		0.674** (0.124)
Married		0.013 (0.021)		0.145 (0.111)
Number of children		0.004 (0.008)		-0.044 (0.042)
Number of adults		-0.048** (0.008)		-0.230** (0.037)
Metropolitan		0.120** (0.040)		0.446* (0.233)
Other Control:	No	Yes	No	Yes
N	3337	3337	3337	3337
Log likelihood	-1646.177	-1580.338	-7355.279	-7299.037
R-squared			0.252	0.277

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: * $p < .10$, ** $p < .05$. The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old. All specifications include basic monthly survey fixed effects and country fixed effects. Specifications 2 and 4 include Census division fixed effects.

The second column presents the specification that includes potentially endogenous control variables. Again, each year of foreign education decreases the probability of having any domestic education by 3.0 percentage points. All else equal, citizens are 9.9 percentage points more likely to have attended school after migration, perhaps because they are more committed to staying in the United States. Residents of metropolitan areas are also 12.0 percentage points more likely to have attended school in the United States, which is likely due to the greater availability of local colleges.

The right panel presents the ordinary least squares regressions with domestic education as the dependent variable. The results are qualitatively similar to the logistic average marginal effect estimates. Each additional year of foreign education is associated with a 0.32 year decrease in domestic education. Potential foreign work experience is also negatively associated with domestic education, with each year of experience being associated with 0.027 less years of domestic education. The results are similar when I include the set of potentially endogenous control variables. Like the logistic regression average marginal effects, citizenship and metropolitan residence are positively associated with years of domestic education.

One concern with defining foreign education as the difference between total education and domestic education is presence of classical measurement error in total education and domestic education (Barry R. Chiswick and Paul W. Miller 1994). This may be due to the interval reporting of total education in the basic monthly survey, part-time school enrollment, or grade retention. In this case, the coefficient on foreign education is biased downward as shown in Equation (13), where θ_d is the measurement error in domestic education. The problem here is that the variance of the measurement error may be large enough to produce a negative relationship between foreign education and domestic education in the data when the actual relationship is positive.

$$p \lim \hat{\phi}_1 = \tau_1 \frac{\text{var}(E_d)}{\text{var}(E_d) + \text{var}(\theta_d)} - \frac{\text{var}(\theta_d)}{\text{var}(E_d) + \text{var}(\theta_d)} \quad (13)$$

To assess the importance of this problem, I use a reliability ratio of 0.88 from Ashenfelter and Card (1994) and estimate that the variance of the measurement error in domestic education is 0.88.¹⁶ Equation (13) implies that the coefficient on foreign education without measurement error is equal to -0.27, compared to -0.32 with the measurement error. While there is attenuation bias in the first term, the second term decreases the coefficient estimate by 0.12. Thus, classical measurement error in total education and domestic education does bias the relationship between domestic education and foreign education, but it is not a critical problem because the absolute magnitude of the bias is small.

In results not shown here but available upon request, I estimate specifications that control for race and ethnicity fixed effects and find similar results. I also estimate regressions with domestic education as the dependent variable among immigrants who have attended school in the United States. Since the results are qualitatively similar to those in the left panel of Table 13, I estimate tobit models and find again that foreign education and domestic education are negatively correlated. In these specifications, each year of foreign education is associated with a decrease of almost half a year of domestic education.

Overall, the results indicate that domestic education is indeed endogenous. Consistent with two of the three previous studies of immigrants in the United States (George J. Borjas 1982; Aliya Hashmi Khan 1997), I show that the relationship between foreign education and domestic education is negative – less-skilled immigrants have more domestic education than highly-skilled immigrants, all else

¹⁶ The variance of the measurement error is $(1-0.88)/0.88 \times 2.5362 = 0.88$.

equal. This relationship implies that over-controlling for domestic education in wage regressions will lead to upward bias in the return to foreign education.

V. Foreign Education in the United States Labor Market

This section presents the biased and unbiased estimates of the return to foreign education. Column (1) of Table 14 presents the estimates for natives as a comparison to the immigrant sample. The return to domestic education is 11.3 percent, which is in line with the previous estimates in the literature. There is also a positive return to potential domestic work experience at 0.90 percent per year. The second column adds the potentially endogenous control variables, and as expected, married men and men living in metropolitan areas earn higher wages than single men and men outside metropolitan areas, respectively. The return to domestic education in this specification is largely unchanged at 10.4 percent.

How do the estimates from the CPS compare to previous estimates of the return to foreign education? Column (3) replicates the standard specification in previous studies by using the piecewise function of total education and age at arrival to compute foreign education. Because the sample consists of immigrants whose age at arrival is at least 25 years old, the piecewise function assumes that total education is foreign education. In this case, the return to foreign education is 5.8 percent, which is less half than the return domestic education for natives. This estimate is in line with previous estimates based on data from the U.S. Census (Julian R. Betts and Magnus Lofstrom 2000; Barry R. Chiswick 1978; Robert F. Schoeni 1997). Each additional year since migration is associated with a 1.1 percent wage increase, although this is biased upward due to emigration bias (Darren Lubotsky 2007). In contrast to previous studies (George J. Borjas 1985; George J. Borjas 1995), there is no evidence of cohort differences in wages, although this is likely due to small sample size. Controlling for potentially endogenous control variables in column (4) does not have a sizable effect

on the return to foreign education. Among the additional control variables, married men earn 5.7 percent more than single men, and the point estimates suggest wage premiums for citizen and metropolitan but are not statistically significant.

To understand the bias from the measurement error problem first, columns (5) and (6) estimate unrestricted models that control for foreign education and domestic education without using the piecewise function. In the baseline case, the return to foreign education is 5.4 percent, and under the assumption of exogenous domestic education, the return to domestic education is 6.8 percent. The results show that the measurement error generates upward bias in the return to foreign education because the return to domestic education is greater than the return to foreign education. However, the magnitude of the measurement error bias is small in part because the variance of domestic education is small relative to the variance of foreign education.

The final two columns exclude domestic education from the specification to analyze the remaining bias from including it as an endogenous control variable. Column (7) shows that the corrected return to foreign education is only 3.3 percent in the baseline specification. The overall return to foreign education is lower than the estimates that only correct for measurement error because foreign education and domestic education are negatively associated. As expected, the regression of the wage residuals on the domestic education investment residuals give a coefficient of 0.068, which means that unobservables that lead to greater domestic education also lead to greater wages. Column (8) shows that the corrected return to foreign education is largely unchanged after including the potentially endogenous control variables at 3.1 percent. In this specification, citizenship is associated a 5.9 percent wage increase, as is marital status and metropolitan status at 7.4 percent and 9.7 percent, respectively.

Table 14. Decomposing the Bias in the Return to Foreign Education

	(1)	(2)	(3)	(4)
	ln(Hourly wage)			
	Natives		Immigrants	
Domestic education	0.113** (0.001)	0.104** (0.001)		
Total education			0.058** (0.003)	0.057** (0.003)
Potential domestic work experience	0.009** (0.000)	0.009** (0.000)		
Potential foreign work experience			0.002 (0.001)	0.003* (0.001)
Years since migration			0.011** (0.002)	0.010** (0.002)
Arrived 1900–1969			-0.064 (0.088)	-0.010 (0.088)
Arrived 1970–1989			-0.027 (0.036)	-0.027 (0.036)
Citizen				0.018 (0.025)
Married		0.129** (0.006)		0.057** (0.025)
Number of children		0.033** (0.002)		0.015 (0.010)
Number of adults		-0.027** (0.003)		-0.021** (0.009)
Metropolitan		0.131** (0.006)		0.065 (0.042)
Other Control:	No	Yes	No	Yes
R-squared	0.229	0.287	0.380	0.394

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: * $p < .10$, ** $p < .05$. The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old. All specifications include basic monthly survey fixed effects, and all immigrant specifications include country fixed effects. Specifications 2, 4, 6, and 8 also control for part-time and union coverage and include Census division fixed effects.

Table 14 (Continued).

	(5)	(6)	(7)	(8)
	ln(Hourly wage)			
	Immigrants			
Domestic education	0.068** (0.004)	0.067** (0.004)		
Foreign education	0.054** (0.003)	0.053** (0.003)	0.033** (0.003)	0.031** (0.003)
Potential foreign work experience	0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)
Years since migration	0.010** (0.002)	0.009** (0.002)	0.011** (0.002)	0.008** (0.002)
Arrived 1900–1969	-0.067 (0.087)	-0.013 (0.087)	-0.062 (0.095)	-0.003 (0.094)
Arrived 1970–1989	-0.029 (0.036)	-0.028 (0.036)	-0.033 (0.037)	-0.028 (0.037)
Citizen		0.013 (0.025)		0.059** (0.027)
Married		0.064** (0.025)		0.074** (0.026)
Number of children		0.012 (0.010)		0.009 (0.010)
Number of adults		-0.019** (0.009)		-0.035** (0.009)
Metropolitan		0.067 (0.042)		0.097** (0.044)
Other Control:	No	Yes	No	Yes
R-squared	0.382	0.397	0.326	0.344

Thus, the corrected estimates of the return to foreign education are about half the size of the estimates in previous studies, and only one-third the size of the return to domestic education among natives. These results indicate that foreign education is much less valuable in the United States labor market than previously thought. Part of the difference is that previous studies misattribute domestic education as foreign education, but the more important reason empirically is that the econometric specifications that over-control for domestic education.

Table 15. Non-Linear Returns to Education for Natives and Immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Hourly wage)					
	Natives		Immigrants			
Dom. ed.	0.043**	0.030**				
	(0.006)	(0.006)				
Dom. ed.≥12	-0.693**	-0.767**				
	(0.066)	(0.063)				
Dom. ed. *	0.072**	0.077**				
Dom. ed.≥12	(0.007)	(0.006)				
Tot. ed.			0.018**	0.015**		
			(0.005)	(0.005)		
Tot. ed.≥12			-1.069**	-1.108**		
			(0.084)	(0.084)		
Tot. ed. *			0.092**	0.096**		
Tot. ed.≥12			(0.007)	(0.007)		
For. ed.					0.010**	0.007
					(0.005)	(0.005)
For. ed.≥12					-0.891**	-0.896**
					(0.099)	(0.099)
For. ed. *					0.073**	0.075**
For. ed.≥12					(0.008)	(0.008)
Other Control:	No	Yes	No	Yes	No	Yes
R-squared	0.232	0.290	0.415	0.431	0.349	0.367

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: * $p < .10$, ** $p < .05$. The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old. All specifications include basic monthly survey fixed effects, all native specifications control for potential domestic work experience, and all immigrant specifications control for potential foreign work experience and years since migration and include cohort and country fixed effects. Specifications 2, 4, and 6 also control for citizen (only for immigrants), married, number of children, number of adults, metropolitan, part-time and union coverage and include Census division fixed effects. Dom. ed. is domestic education, Tot. ed. is total education, and for. ed. is foreign education.

Table 16. Quantile Regression Estimates for the Return to Education

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Hourly wage)					
	Natives			Immigrants		
	25th	50th	75th	25th	50th	75th
Domestic education	0.093** (0.001)	0.107** (0.001)	0.119** (0.001)			
Foreign education				0.014** (0.003)	0.031** (0.003)	0.034** (0.002)
Potential total exp.	0.007** (0.000)	0.010** (0.000)	0.011** (0.000)			
Potential foreign exp.				-0.004** (0.001)	-0.001 (0.002)	0.001 (0.001)
Years since migration				0.008** (0.002)	0.007** (0.003)	0.007** (0.001)
Arrived 1900–1969				-0.035 (0.083)	0.091 (0.100)	0.066 (0.051)
Arrived 1970–1989				-0.041 (0.035)	-0.002 (0.041)	-0.010 (0.022)
Citizen				0.058** (0.024)	0.080** (0.028)	0.065** (0.015)
Married	0.145** (0.008)	0.126** (0.007)	0.117** (0.007)	0.048** (0.024)	0.054* (0.028)	0.079** (0.015)
Number of children	0.034** (0.003)	0.035** (0.003)	0.031** (0.003)	0.012 (0.010)	0.010 (0.011)	0.019** (0.006)
Number of adults	-0.031** (0.004)	-0.026** (0.004)	-0.023** (0.004)	-0.016* (0.008)	-0.028** (0.010)	-0.038** (0.006)
Metropolitan	0.116** (0.007)	0.140** (0.007)	0.140** (0.007)	0.124** (0.042)	0.028 (0.051)	0.089** (0.028)
Other Controls:	Yes	Yes	Yes	Yes	Yes	Yes

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: * $p < .10$, ** $p < .05$. The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 25 years old. All specifications include basic monthly survey fixed effects, and all immigrant specifications include country fixed effects. All specifications include Census division fixed effects.

Table 17. Sample Restrictions and Corrections for Attrition Bias

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(Hourly wage)					
	Age at arrival ≥ 16			Age at arrival ≥ 25		
	All	Potential foreign exp. >0	Potential foreign exp.>5	All	October only	Inverse probability weights
Foreign education	0.024** (0.002)	0.023** (0.002)	0.023** (0.003)	0.038** (0.003)	0.037** (0.006)	0.037** (0.003)
Potential foreign exp.	-0.002* (0.001)	-0.002 (0.001)	-0.002 (0.001)	0.000 (0.002)	0.001 (0.003)	-0.000 (0.002)
Years since migration	0.005** (0.002)	0.005** (0.002)	0.007** (0.002)	0.008** (0.002)	0.005 (0.004)	0.008** (0.002)
Arrived 1900–1969	0.035 (0.058)	0.011 (0.062)	-0.045 (0.073)	0.012 (0.096)	0.303* (0.160)	-0.008 (0.096)
Arrived 1970–1989	0.040 (0.026)	0.029 (0.026)	0.006 (0.030)	-0.030 (0.038)	-0.093 (0.067)	-0.028 (0.039)
Citizen	0.086** (0.019)	0.089** (0.020)	0.092** (0.023)	0.054** (0.026)	0.075 (0.049)	0.053** (0.027)
Married	0.110** (0.019)	0.111** (0.020)	0.089** (0.022)	0.099** (0.027)	0.146** (0.050)	0.098** (0.027)
Number of children	-0.005 (0.007)	-0.007 (0.007)	-0.009 (0.008)	0.001 (0.010)	0.020 (0.021)	-0.000 (0.010)
Number of adults	-0.039** (0.006)	-0.040** (0.006)	-0.038** (0.007)	-0.045** (0.009)	-0.048** (0.017)	-0.042** (0.009)
Metropolitan	0.126** (0.029)	0.122** (0.028)	0.106** (0.032)	0.068 (0.044)	-0.041 (0.072)	0.064 (0.045)
Other Control:	Yes	Yes	Yes	Yes	Yes	Yes
Mod. Spec.:	No	No	No	Yes	Yes	Yes
R-squared	0.334	0.325	0.317	0.300	0.338	0.301

Sources: Current Population Survey in 1995, 1996, 1999, 2000, 2004, and 2005.

Notes: * $p < .10$, ** $p < .05$. The sample consists of men in the October Supplements with matches in the basic monthly surveys, between ages 25 and 64 years old, without missing or negative foreign education, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. Natives are U.S. born with U.S. born parents, and immigrants are foreign-born whose ages at arrival are at least 16 or 25 years old. All specifications include basic monthly survey, country, and Census division fixed effects.

As an extension, I test for non-linear returns to education in Table 15.

Conceptually, the return to education may exhibit nonlinearities if degrees signal productivity to employers via “sheepskin effects” or if worker responses to labor supply and demand shocks vary by schooling (James Heckman, Anne Layne-Farrar and Petra Todd 1996; Thomas Hungerford and Gary S. Solon 1987; David A. Jaeger and Marianne E. Page 1996). Empirically, the local linear regressions in Figure 1 suggest that the slope for natives changes at 11 or 12 years of schooling, which corresponds to the schooling required for high school completion. The estimates in columns (1) and (2) confirm the presence of non-linear returns to domestic education for natives, with a return of 4.3 percent for workers with less than 12 years of schooling, compared to a return to 7.2 percent for workers with more than 12 years of schooling.

The return to foreign education among immigrants need not be non-linear if employers do not recognize the “sheepskins” of foreign education systems. Betts and Lofstrom (2000) and Bratsberg and Ragan (2002) both find evidence of a nonlinearity at 12 years of schooling, although both are subject to the measurement error and misspecification problems described above. Using the uncorrected specification for immigrants, the CPS data shows a similar pattern, with the return to foreign education among the less educated at only 1.8 percent and at 9.2 percent for workers with at least 12 years of school. Surprisingly, while foreign education is less valuable than domestic education at the low end of the skill distribution, it is actually more valuable for immigrants with at least 12 years of schooling. When I correct for measurement error and model misspecification, the return to foreign education for workers with less than 12 years of foreign education is 1.0 percent, while the return for workers with at least 12 years of foreign education is 7.3 percent. Thus, the difference in the return to foreign education among immigrants and the return to domestic education among

natives is driven by differences in the value of education among workers with less than 12 years of schooling. For those with at least 12 years of schooling, the return to foreign education among immigrants and the return to domestic education among natives are comparable.

VI. Robustness Exercises

In this section, I present results from four sets of robustness exercises that address some of the potential biases in the return to foreign education. First, I estimate quantile regressions to (1) protect against the bias due to the top-coding of wages and (2) allow for the return to foreign education to vary at various points of the wage distribution. The top-coding of wages may be particularly important here since many highly-skilled immigrants come to the United States under the H1-B visa program, particularly those in the high-paying technology industry.

For comparison, the left panel of Table 16 presents the quantile regressions for natives at the 25th, 50th, and 75th percentiles of the wage distribution. The return to domestic education is 9.3 percent at the 25th percentile, increases to 10.7 percent at the 50th percentile, and then increase to 11.9 percent at the 75th percentile. The right panel presents the estimates for immigrants based on specifications without measurement error from the piecewise function or with domestic education as an endogenous control variable. The return to foreign education at the 25th percentile is 1.4 percent, 3.1 percent at the 50th percentile, and 3.4 percent at the 75th percentile. There are two takeaways from this set of results. First, the return to domestic education among natives and the return to foreign education among immigrants increase with the percentile of the wage distribution. Potential explanations for these trends are that (1) workers at higher percentiles of the wage distribution have greater unobserved ability, which interacts positively with the return to schooling, (2) schooling and unobserved school quality are positively correlated, and (3) workers

with high levels of schooling are employed in jobs with low skill requirements (Pedro S. Martins and Pedro T. Pereira 2004). Second, the estimates at the 50th percentile are similar to those from the ordinary least squares specifications, which indicate that the top-coding of wages does not cause any quantitatively meaningful bias in the returns to domestic and foreign education.

The second robustness check is based on the selection of immigrants with uncensored foreign education. Up to this point, the immigrant sample consists of people who were at least 25 years old when they arrived in the United States. However, many immigrants come at earlier ages but still have uncensored foreign education. In the absence of data on immigrant visa type, there are two available alternatives using the CPS. The first approach is to reduce the age at arrival criteria to allow additional immigrants into the sample. The left panel of Table 17 shows the results using 16 years old at the time of migration as the new lower boundary. As shown in column (1), the return to foreign education in the expanded sample is 2.4 percent, which is lower than the return in the main sample. This result indicates that the return to foreign education increases with age at arrival, which suggests that the timing of migration is endogenous.

Because the expanded sample also adds immigrants with interrupted education, I restrict the sample further by imposing a potential foreign work experience condition. The assumption here is that immigrants have continuous schooling and complete their foreign education once they take a break. Column (2) shows results for immigrants who were at least age 16 years old when they arrived in the United States and who have at least one year of potential foreign work experience. The return to foreign education is 2.3 percent, which is the same as the return without the experience constraint. Expanding the criteria to more than five years of potential work experience does not change the estimated return to foreign education.

The third exercise is designed to study the panel attrition bias due to non-matches between the October Supplements to the basic monthly surveys. Respondents may not have wage data in November, December, and January months due to migration, death, or non-response (2000). One solution is to limit the sample to only the October basic monthly survey since panel attrition would then not be an issue. A second solution is to reweight the October, November, December, and January samples to match the size of the original October sample, but doing so requires a modified specification with an abbreviated set of country fixed effects and without controlling for union coverage. For comparison, column (4) presents the usual sample consisting of immigrants whose age at arrival is at least 25 years old with the modified specification. The results indicate a slightly higher return to foreign education at 3.8 percent. Column (5) shows the results for the Outgoing Rotation Groups in October of 1995, 1999, and 2004 only. In this case, the return to foreign education is quantitatively similar at 3.7 percent. The final column multiplies the sampling weights by inverse probability weights to correct for panel attrition bias. Again, column (6) shows no effect of missing wage data on the estimated return to foreign education.

The final robustness check assesses how classical measurement error affects the return to foreign education, both compared to previous studies and in the non-linear schooling specifications. First, I calculate the return to foreign education after accounting for classical measurement error in both total education and domestic education. As shown in Equation (14), the probability limit of the coefficient on foreign education exhibits attenuation bias similar to the usual measurement error setup. Note that the variance of foreign education with measurement error is given in Equation (15). With an estimated return to foreign education with error at 3.3 percent, the true return to foreign education is slightly greater at 4.0 percent. Thus, while

classical measurement error in total education and domestic education lead to attenuation bias in the return to foreign education, correcting for measurement error and misspecification bias still generates a return that is lower than estimates in previous studies and estimates of the return to domestic education among natives.

$$p \lim \hat{\phi}_1 = \beta_1 \frac{\text{var}(T - E_d)}{\text{var}(T - E_d) + \text{var}(\theta_T) + \text{var}(\theta_d)} \quad (14)$$

$$\text{var}(T + \theta_T - E_d - \theta_d) = \text{var}(T - E_d) + \text{var}(\theta_T) + \text{var}(\theta_d) \quad (15)$$

The second part of the exercise addresses the variation in classical measurement error as a function of total education. As discussed above, respondents to the CPS basic monthly survey report total education as either 0 years, 1 to 4 years, 5 to 6 years, 7 to 8 years, 9 years, 10 years, 11 years, 12 years, some college, undergraduate, or advanced degree. This means that the measurement error in total education is largest for those with the lowest levels of total education. Figure 5 shows the cumulative distribution function of schooling and demonstrates that while workers with less than eight years of school make up only 1.2 percent of natives, it makes up 18.7 percent of immigrants. Thus, one possible explanation for the difference between the return to foreign education and the return to domestic education among natives, as well as the nonlinearity of the return to foreign education among immigrants, is measurement error at the low end of the distribution of total education.

It is difficult to get a convincing handle on the measurement error due to the interval-reporting of total education versus the measurement error from all other causes.¹⁷ Instead, I argue that it is unlikely that (1) the return to foreign education equals the return to domestic education among immigrants, and that (2) the return to

¹⁷ Unfortunately, the data used by Jaeger (1997) does not permit the disaggregation by nativity.

foreign education among workers with less than 12 years of schooling equals the return to foreign education among workers with at least 12 years of schooling. The first case requires a reliability ratio of 0.29, and the second case requires a reliability ratio of 0.14, both of which are arguably implausible given that the overall reliability ratio of total education is three and six times the size these requirements, respectively (Orley C. Ashenfelter and Alan B. Krueger 1994).

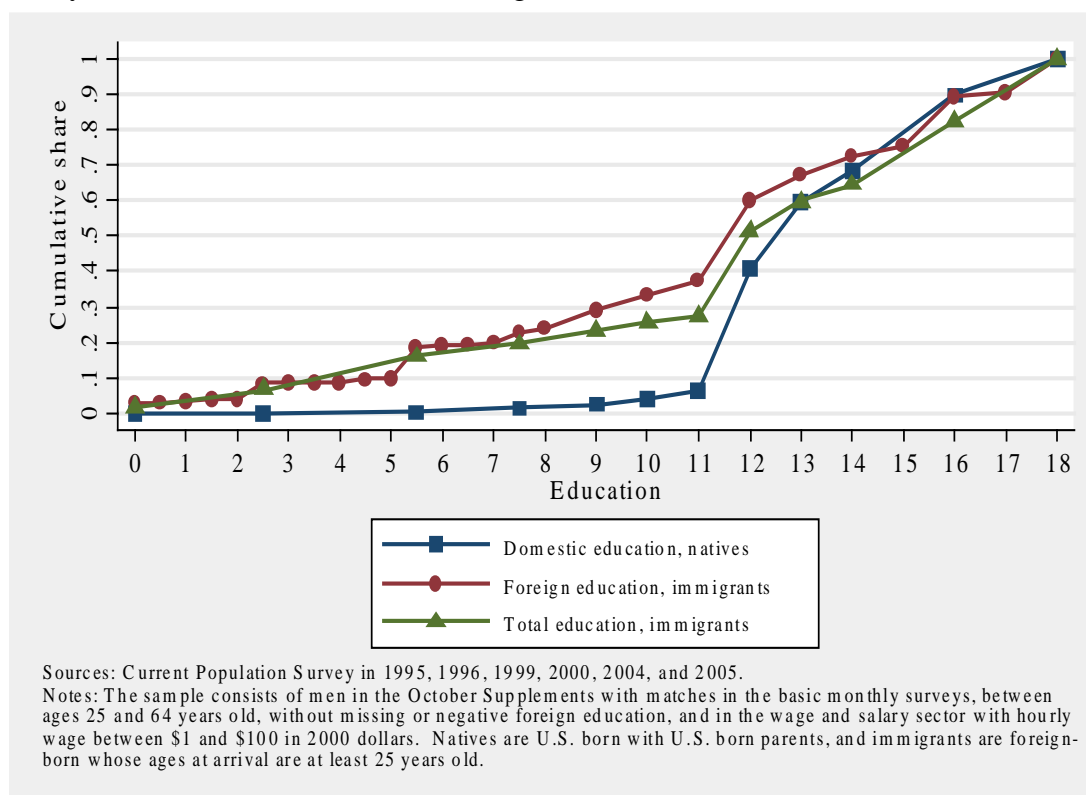


Figure 5. Cumulative Distribution Functions for Education

VII. Implications for Labor Economics and Immigration Policy

The results of this paper show that the overall return to foreign education among immigrants is 3.3 percent per year. This rate of return is less than one-third the return to domestic education among natives at 11.3 percent, which suggests that the foreign education is not as valued in the domestic labor market. These results are robust to a variety of specifications that address the potential biases from the top-

coding of wages, the selection of immigrants with uncensored foreign education, panel attrition in the CPS, and classical measurement error in domestic education and total education.

My estimates are substantially lower than those in previous studies for two reasons. First, previous studies calculate foreign education using a piecewise function of total education and age at arrival. This approach generates upward bias in the return to foreign education because it misclassifies relatively more valuable domestic education as foreign education. The size of the bias is small, partly because the difference in the returns to education is small and partly because the variance of domestic education is small relative to the variance of foreign education.

The second reason why previous studies produce greater returns to foreign education is that they misspecify the econometric model by controlling for endogenous domestic education. Part of the return to foreign education operates through additional investment after migration, and our research interest is in the overall value of foreign education the domestic labor market. The CPS data show that it is immigrants with less foreign education who make greater investments in post-migration human capital. The negative correlation between foreign education and domestic education generates substantial upward bias in the return to foreign education when the specification over-controls for domestic education.

Notably, non-linear specifications of foreign education indicate that the difference in the returns to education between immigrants and natives occur among workers with less than 12 years of school. In this range, the return to domestic education among natives is 4.3 percent, while the corrected return to foreign education among immigrants is only 1.0 percent. Surprisingly, the non-linear specifications suggest that the two returns to education among workers with at least 12 years of school are actually equal. Subsequent studies will need to assess the extent to which

the observed differences in the returns to education are due to classical measurement error from interval-reported schooling.

With this caveat in mind, the results have important implications for our understanding of the labor market. A major cause of the immigrant-native wage gap is that foreign education among immigrants is less valued than domestic education among natives. There are several potential explanations for this empirical result. It may be that curriculums in foreign countries are not applicable in the domestic labor market, or that even with the same curriculum, schools in other countries are less effective than those in the United States. An additional explanation is that employers are unable to recognize the quality of education from unfamiliar, foreign schools.

The results also have important implications for federal immigration policy. Currently, policymakers are considering proposals to increase the fraction of immigrants admitted on the basis of human capital. This proposal is motivated by concern over the net fiscal burden of immigration. Proponents argue that less-educated immigrants are more likely to rely on social services, so admitting highly-educated immigrants decreases the net fiscal cost of immigration. The results in this paper make two contributions to this policy arena. First, selecting immigrants on the basis of foreign education is less likely to lead to higher immigrant wages than previously thought. And second, the evidence in this paper suggests that this proposal is particularly unlikely to work among immigrants with less than 12 years of foreign education.

VIII. Technical Appendix

A. Correct Wage Specification

Assume that the true model is Equation (A1) and that foreign education and the error term are uncorrelated. Then the usual ordinary least squares estimator of the return to foreign education is unbiased.

$$Y = \alpha_0 + E_f \alpha_1 + \varepsilon \quad (\text{A1})$$

$$\begin{aligned} p \lim \hat{\alpha}_1 &= \frac{\text{cov}(Y, E_f)}{\text{var}(E_f)} \\ &= \frac{\text{cov}(\alpha_0 + E_f \alpha_1 + \varepsilon, E_f)}{\text{var}(E_f)} \\ &= \alpha_1 + \frac{\text{cov}(\varepsilon, E_f)}{\text{var}(E_f)} \\ &= \alpha_1 \end{aligned} \quad (\text{A2})$$

B. Measurement Error and Exogenous Domestic Education

In the measurement error case in Equation (A3) and Equation (A4), the econometric specification is still a regression of wages on foreign education. However, the proxy for foreign education is total education since the piecewise function assumes that all education for immigrants whose ages at arrival are at least 25 years old is of foreign origin.

$$Y = \delta_0 + \delta_1 E_f^* + \nu \quad (\text{A3})$$

$$E_f^* = T = E_f + E_d \quad (\text{A4})$$

The bias in this specification is best understood by studying the regression of wages on both foreign education and domestic education shown in Equation (A5). First, note that the probability limit of the return to domestic education is given in Equation (A6).

$$Y = \alpha_0 + \alpha_1 E_f + \alpha_2 E_d + \omega \quad (\text{A5})$$

$$p \lim \hat{\alpha}_2 = \frac{\text{cov}(Y, E_d)}{\text{var}(E_d)} \quad (\text{A6})$$

Thus, the probability limit of the measurement error specification is given in Equation (A7).

$$\begin{aligned}
p \lim \hat{\delta}_1 &= \frac{\text{cov}(Y, E_f^*)}{\text{var}(E_f^*)} \\
&= \frac{\text{cov}(Y, E_f + E_d)}{\text{var}(E_f^*)} \\
&= \frac{\frac{\text{cov}(Y, E_f)}{\text{var}(E_f)} \text{var}(E_f) + \frac{\text{cov}(Y, E_d)}{\text{var}(E_d)} \text{var}(E_d)}{\text{var}(E_f) + \text{var}(E_d)} \\
&= \frac{\alpha_1 \text{var}(E_f) + \alpha_2 \text{var}(E_d)}{\text{var}(E_f) + \text{var}(E_d)} \\
&= \alpha_1 + (\alpha_2 - \alpha_1) \frac{\text{var}(E_d)}{\text{var}(E_f) + \text{var}(E_d)}
\end{aligned} \tag{A7}$$

C. Domestic Education as an Endogenous Control Variable

In the model misspecification case in Equation (A8), domestic education is included as a control variable that is endogenous. The endogeneity of domestic education is shown in Equation (A9).

$$Y = \lambda_0 + E_f \lambda_1 + E_d \lambda_2 + \psi \tag{A8}$$

$$E_d = \tau_0 + \tau_1 E_f + \nu \tag{A9}$$

Using the derivation from Angrist and Krueger (1999), the first step is to set up a partitioned regression using Equation (A10) and Equation (A11).

$$E_f = \eta_0 + \eta_1 E_d + \rho \tag{A10}$$

$$Y = \pi_0 + \pi_1 E_d + \kappa \tag{A11}$$

Note that these two equations generate Equation (A12) and Equation (A13).

$$\begin{aligned}
p \lim \hat{\eta}_1 &= \frac{\text{cov}(E_f, E_d)}{\text{var}(E_d)} \\
&= \tau_1 \frac{\text{var}(E_f)}{\text{var}(E_d)}
\end{aligned} \tag{A12}$$

$$\begin{aligned}
\text{var}(\rho) &= \text{var}(E_f - \eta_0 - \eta_1 E_d) \\
&= \text{var}(E_f) - \eta_1^2 \text{var}(E_d) \\
&= \text{var}(E_f) - \tau_1^2 \frac{\text{var}^2(E_f)}{\text{var}^2(E_d)} \text{var}(E_d) \\
&= \frac{\text{var}(E_f)}{\text{var}(E_d)} (\text{var}(E_d) - \tau_1^2 \text{var}(E_f)) \\
&= \frac{\text{var}(E_f)}{\text{var}(E_d)} \text{var}(\nu)
\end{aligned} \tag{A13}$$

Lastly, the probability limit of the return to foreign education is given in Equation (A14).

$$\begin{aligned}
p \lim \hat{\lambda}_1 &= \frac{\text{cov}(\kappa, \rho)}{\text{var}(\rho)} \\
&= \frac{\text{cov}(Y - \pi_0 - \pi_1 E_d, \rho)}{\text{var}(\rho)} \\
&= \frac{\text{cov}(Y, \rho)}{\text{var}(\rho)} \\
&= \frac{\text{cov}(\alpha_0 + \alpha_1 E_f + \varepsilon, \rho)}{\text{var}(\rho)} \\
&= \frac{\text{cov}(\alpha_0 + \alpha_1 (\eta_0 + \eta_1 E_d + \rho) + \varepsilon, \rho)}{\text{var}(\rho)} \\
&= \alpha_1 + \frac{\text{cov}(\varepsilon, \rho)}{\text{var}(\rho)} \\
&= \alpha_1 + \frac{\text{cov}(\varepsilon, E_f - \eta_0 - \eta_1 E_d)}{\text{var}(\rho)} \\
&= \alpha_1 - \eta_1 \frac{\text{cov}(\varepsilon, E_d)}{\text{var}(\rho)} \\
&= \alpha_1 - \eta_1 \frac{\text{cov}(\varepsilon, \tau_0 + \tau_1 E_f + \nu)}{\text{var}(\rho)} \\
p \lim \hat{\lambda}_1 &= \alpha_1 - \eta_1 \frac{\text{cov}(\varepsilon, \nu)}{\text{var}(\rho)} \\
&= \alpha_1 - \tau_1 \frac{\text{var}(E_f)}{\text{var}(E_d)} \text{cov}(\varepsilon, \nu) \frac{\text{var}(E_d)}{\text{var}(E_f)} \frac{1}{\text{var}(\nu)} \\
&= \alpha_1 - \tau_1 \frac{\text{cov}(\varepsilon, \nu)}{\text{var}(\nu)}
\end{aligned} \tag{A14}$$

D. Classical Measurement Error in Investment in Domestic Education

In this case, there is classical measurement error for both total education and for domestic education. The econometric specification consists of Equation (A15), and the classical measurement error for domestic education and foreign education are given in Equation (A16) and Equation (A17), respectively.

$$E_d^* = \phi_0 + \phi_1 E_f^* + \xi \quad (\text{A15})$$

$$E_d^* = E_d + \theta_d \quad (\text{A16})$$

$$E_f^* = (T + \theta_T) - (E_d^* + \theta_d) \quad (\text{A17})$$

Thus, the probability limit of the coefficient on foreign education is:

$$\begin{aligned} p \lim \hat{\phi}_1 &= \frac{\text{cov}(E_f^*, E_d^*)}{\text{var}(E_d^*)} \\ &= \frac{\text{cov}(T + \theta_T - E_d - \theta_d, E_d + \theta_d)}{\text{var}(E_d + \theta_d)} \\ &= \frac{\text{cov}(T - E_d, E_d) - \text{var}(\theta_d)}{\text{var}(E_d) + \text{var}(\theta_d)} \\ &= \tau_1 \frac{\text{var}(E_d)}{\text{var}(E_d) + \text{var}(\theta_d)} - \frac{\text{var}(\theta_d)}{\text{var}(E_d) + \text{var}(\theta_d)} \end{aligned} \quad (\text{A18})$$

E. Classical Measurement Error in the Wage Specification

Assume that the classical measurement error for foreign education is that in Equation (A17). Then the regression consists of Equation (A19), and the probability limit of the return to foreign education is shown in Equation (A20).

$$Y = \varphi_0 + \varphi_1 (T + \theta_T - E_d - \theta_d) + \varepsilon \quad (\text{A19})$$

$$\begin{aligned} p \lim \hat{\varphi}_1 &= \frac{\text{cov}(Y, T + \theta_T - E_d - \theta_d)}{\text{var}(T + \theta_T - E_d - \theta_d)} \\ &= \frac{\text{cov}(Y, T - E_d)}{\text{var}(T - E_d)} \frac{\text{var}(T - E_d)}{\text{var}(T + \theta_T - E_d - \theta_d)} \\ &= \beta_1 \frac{\text{var}(T - E_d)}{\text{var}(T - E_d) + \text{var}(\theta_T) + \text{var}(\theta_d)} \end{aligned} \quad (\text{A20})$$

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CHAPTER 3

DO COUNTRY CHARACTERISTICS REALLY MEASURE THE QUALITY OF FOREIGN SCHOOLS?

I. Introduction

Does school quality cause greater wages in the labor market? Previous studies that use educational expenditure and class size as proxies for United States school quality have yet to reach a consensus. On the one hand, Card and Krueger (1992) present evidence that the two measures are positively associated with the return to education at the state level. On the other hand, individual-level studies show that the return to education does not vary with the educational expenditure and class size of the school or district actually attended by the individual (Julian R. Betts 1996; Jeff Grogger 1996).

In contrast, recent studies of the return to foreign education among immigrants have been consistent in concluding that it varies with the quality of foreign schools. Individual-level wage regressions show that the return to foreign education increases with educational expenditures and student-teacher ratios at the country level (Julian R. Betts and Magnus Lofstrom 2000; Bernt Bratsberg and James F. Jr Ragan 2002). Similarly, two-step estimators show that the return to foreign education across countries varies with these two measures (Bernt Bratsberg and Dek Terrell 2002).

A potential problem in the foreign school quality literature is the presence of country-level “cultural” unobservables that are correlated with school quality and wages, which may lead to spurious estimates of the return to the quality of foreign education. This problem is an example of Hanushek, Rivkin, and Taylor’s (1996) critique that aggregate-level unobservables affect the interpretation of aggregate-level proxies of school quality. When comparing immigrants across countries, cultural unobservables may take the form of early health outcomes that are associated with

educational expenditures but also independently raise the return to foreign education. Alternatively, they may be an artifact of selective migration, in which the average school quality in the country is not the average school quality among people who actually come to the United States. Cultural unobservables are more problematic here than in studies of domestic schools if the differences in unobservables between countries are greater than the differences in within each country.

In this paper, I test the extent to which educational expenditures as a share of GDP per capita and student-teacher ratios at the country level can be interpreted as measures of foreign school quality. I also examine whether math and science student achievement test scores at the country level, defined as labor force quality by Hanushek and Kimko (2000), can be viewed as a proxy of school quality. My identification strategy compares the effect of these measures on immigrants who arrived as adults with immigrants who arrived as children. The three measures encompass both school quality and cultural unobservables in the first group, but they only reflect cultural unobservables in the second group because these immigrants never actually attended school in their countries of birth. This test formalizes Hanushek and Kimko's (2000) comparison of the effect of labor force quality on immigrants with and without United States education. It is similar in spirit to Bleakley and Chin (2004) who use of age at arrival and country of origin to identify variation in English language skills.

Using data from the 2000 Census and the 2005 American Community Survey, I confirm the results of previous studies by showing that the return to foreign education is associated with the three potential measures of foreign school quality in the expected directions. However, the results persist only for educational expenditures once I properly account for the country-level correlation of the wage residuals. The falsification tests using child-arriving immigrants show even stronger effects of the

three potential measures of foreign school quality, which suggests that they also reflect cultural unobservables. Triple difference specifications that separately identify the effects of foreign school quality and cultural unobservables show that all three of the country characteristics are measures of cultural unobservables more than foreign school quality.

The remainder of this paper is structured as follows. In Section II, I discuss the literature on the returns to school quality, and in section III, I describe the data and identification strategy. Section IV presents the empirical results, and section VI concludes.

II. Literature Review

Variation in the quality of educational institutions may generate heterogeneity in the return to education (Jere R. Behrman and Nancy Birdsall 1983). An ideal research design to test this hypothesis is to randomly assign children to schools and estimate school quality as the school's value-added contribution to student outcomes. Then, the causal effect of school quality is estimated by regressing subsequent labor market outcomes on school quality. Because school fixed-effects estimates from value-added models are usually unavailable, scholars typically use school, district, or state characteristics – such as student-teacher ratios and expenditures per pupil – as inputs in education production functions that predict school quality. However, there continues to be substantial disagreement on whether these measures are actually valid proxies of school quality (for a review, see Eric A. Hanushek 1986).

The literature on immigrant labor market outcomes has addressed differences in foreign school quality since Chiswick's (1978) seminal article on the return to foreign and domestic education. Subsequent articles have taken advantage of data improvements to show that the return to foreign education varies by country of origin and that on average it is less than the return to domestic education among natives (e.g.

Rachel M. Friedberg 2000; Albert Yung-Hsu Liu 2009). Scholars commonly argue that the difference in the returns is due the production of country-specific knowledge by foreign schools, which makes education not entirely portable between countries.

To study the source of the variation in the return to foreign education, scholars have turned to country-level educational inputs as potential measures of school quality. Individual-level wage regressions show statistically significant interactions between the return to schooling and (1) expenditure per pupil as a share of GDP per capita and (2) student-teacher ratios (Julian R. Betts and Magnus Lofstrom 2000; Bernt Bratsberg and James F. Jr Ragan 2002). These studies either estimate naïve ordinary least squares regressions or account for the correlation of wages within country with a random effects specification. An alternative econometric strategy is a two-step estimator that estimates the return to education for each country and then regresses the returns on country characteristics, similar to Card and Krueger's (1992) study of United States school quality at the state level and Borjas' (1987) study of selective migration across countries. In this case, Bratsberg and Terrell (2002) show that the return to foreign education increases with (1) expenditure per pupil as a share of GDP per capita and (2) student-teacher ratios.

While these studies demonstrate that the return to foreign education varies with the potential measures of school quality, they are subject to at least three limitations. First, previous studies typically do not correctly account for the correlation of wages within countries. Using aggregate measures on micro-level data leads to inflated t-statistics in ordinary least squares specifications (Brent R. Moulton 1990). While some studies account for this problem with country random effects, any correlation between the random country-level intercepts and the control variables would lead to inconsistent estimators.

Second, there are likely to be cultural unobservables that vary between

countries, which independently lead to greater returns to education. These unobservables fall into one of two categories. The first are unobserved characteristics shared among all people from a source country, such as the perceived value of education or the match between the curriculum of foreign schools and the demands of the United States labor market. The second class consists of characteristics shared among immigrants from a country that choose to migrate. Cultural unobservables that are correlated with the potential measures of school quality and with wages will lead to spurious effects on the return to foreign education. This problem has been shown to affect the interpretation of state-level studies of school quality and returns to education in the United States (Eric A. Hanushek, Steven G. Rivkin and Lori L. Taylor 1996).

A third limitation is that student-teacher ratios and expenditures per pupil may not be valid predictors of school quality. Indeed, Hanushek (1986) argues that the literature does not reveal an empirical relationship between class size and expenditures per pupil with student achievement in United States public schools. A relatively new approach is to use student outcomes as an alternative measure of school quality. In the domestic education literature, Murphy and Peltzman (2004) relate state average AFQT test scores with state-level earnings. In the foreign education literature, Hanushek and Kimko (2000) develop a measure of labor force quality based on internationally comparable math and science test scores. Under the assumption of stationary school quality, the authors take the average scores of tests administered by the International Association for the Evaluation of Educational Achievement and the International Assessment of Education Progress. Countries who do not give the test to their students are assigned values based on country-level education production functions. The authors then argue that country-level labor force quality is associated with greater earnings among immigrants and thus reflects labor market productivity.

III. Data and Research Design

A. Data

In this paper, I test whether the return to foreign education varies with educational expenditure as a share of GDP, student-teacher ratio, and labor force quality by linking individual-level data on immigrants and natives in the United States with country-level data. I draw the individual-level data from the 2000 5 percent sample United States Census and the 2005 1 percent ACS public use microdata samples. To make the sample sizes manageable, I take an additional 1/10 subsample of all-white native households in the Census and a 1/2 subsample of all-white native households in the American Community Survey and reweight the observations accordingly. The datasets contain information on total education and wages, as well as demographic controls such as country of birth and year of arrival. I restrict the regression sample to men to abstract from selective labor force participation among women. I also exclude respondents who are living in group quarters. I apply a consistent top code set as the minimum top code in the Census and the ACS in 2000 dollars, and then impute wages as 1.4 times the new top code. I divide annual wages and salary by usual work hours and weeks worked to get an estimate of hourly wages. Lastly, I limit the sample to respondents who worked last year in the wage and salary sector and earned between 1 and 100 dollars in 2000 dollars.

Country characteristics data comes from several different sources. Most country characteristics are taken from Blau, Kahn, and Papps (2008), with some minor modifications. The percent refugee data is annual rather than calculated at five year intervals. Student-teacher ratios from Barro and Lee (1994) and the World Bank Development Indicators (2005) and data on primary expenditures as a fraction of GDP per capita from the World Bank Development Indicators. I use the labor force quality variable (QL2*) provided in the appendix of Hanushek and Kimko (2000).

Table 18. Sample Means and Standard Deviations by Age at Arrival Cohort

	(1)	(2)		(3)	(4)
	Adult	Child		Adult	Child
ln(Hourly wage)	2.597 (0.717)	2.838 (0.625)	Arrived 1961–1970	0.019 (0.137)	0.322 (0.467)
Hourly wage	17.356 (13.265)	20.499 (12.479)	Arrived 1971–1980	0.105 (0.307)	0.348 (0.476)
Labor hours per week	42.786 (9.832)	44.242 (10.266)	Arrived 1981–1990	0.259 (0.438)	0.054 (0.225)
Education	12.096 (4.805)	13.719 (2.685)	Arrived 1991–2000	0.456 (0.498)	0.000 (0.000)
White	0.276 (0.447)	0.538 (0.499)	Arrived 2001–2005	0.161 (0.367)	0.000 (0.000)
Black	0.051 (0.220)	0.034 (0.182)	GDP per capita in 1000s	5.342 (6.720)	6.203 (4.833)
Hispanic	0.305 (0.460)	0.204 (0.403)	Student-teacher ratio	29.809 (8.961)	31.921 (10.360)
Asian or Pacific Islander	0.276 (0.447)	0.132 (0.338)	Education exp. ratio	11.663 (5.473)	11.637 (5.607)
Other race	0.092 (0.289)	0.092 (0.289)	English speaking	0.095 (0.294)	0.139 (0.346)
Potential total work exp.	25.536 (10.952)	18.596 (9.425)	Other English	0.132 (0.338)	0.062 (0.242)
Years since migration	10.809 (8.344)	35.869 (9.523)	Non English	0.773 (0.419)	0.799 (0.401)
Age	43.632 (9.596)	38.315 (9.240)	Labor force quality 1	0.397 (0.115)	0.458 (0.109)
Arrived 1900–1950	0.000 (0.000)	0.056 (0.230)	Labor force quality 2	0.428 (0.136)	0.473 (0.112)
Arrived 1951–1960	0.000 (0.000)	0.221 (0.415)	N	145707	41007

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: The sample consists of men, between ages 25 and 64 years old, born in countries with valid source country characteristics, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. The age at arrival for adult arriving immigrants is at least 25 years old, and the age at arrival for child arriving immigrants is less than seven years old.

$$E_f = \begin{cases} T & \text{if } T + 6 \leq A \\ A - 6 & \text{if } 6 < A < T + 6 \\ 0 & \text{if } A \leq 6 \end{cases}$$

(16)

I divide the immigrant sample into three cohorts based on age at arrival. The adult-arriving cohort consists of immigrants whose age at arrival is at least 25 years old. Previous studies decompose education by its country of origin using the piecewise function of total education and age at arrival presented in Equation (16). The function implies that all education of the adult-arriving cohort was earned abroad. For a discussion of the measurement error associated with this approach, see Liu (2009). The second cohort consists of child-arriving immigrants whose age at arrival is less than seven years old. The piecewise function assumes that the education of these immigrants was earned only in the United States. The remaining immigrants in the adolescent-arriving cohort are educated partially in the United States and partially in their countries of birth. I exclude this group of immigrants from the sample since it is impossible to know how much foreign education they would have earned had they not migrated to the United States.

I present the sample means and standard deviations by age at arrival cohort in Table 18. The average hourly wage among adult-arriving immigrants in the sample is 17.4 dollars in 2000 dollars and the average labor supply is full-time. The average grade completed is 12.1 years, which I assume is all of foreign origin. The average years since migration is 10.8 years, which is measured with less error than in previous studies because the 2000 Census and the American Community Survey have immigrants report their year of arrival in years rather than in year intervals as was done in previous Censuses.

The sample statistics for the child-arriving cohort show some important differences. The average wage of this group is 20.5 dollars, which is larger than the average wage among adult-arriving immigrants. While their average labor supply is similar, child-arriving immigrants have almost two additional years of education. Based on the piecewise function to decompose education by its country of origin, total

education for these immigrants is all earned in the United States since they migrated before they were seven years old. In addition, child-arriving immigrants come from earlier year of arrival cohorts on average since the sample is restricted to consist of immigrants between ages 25 and 64.

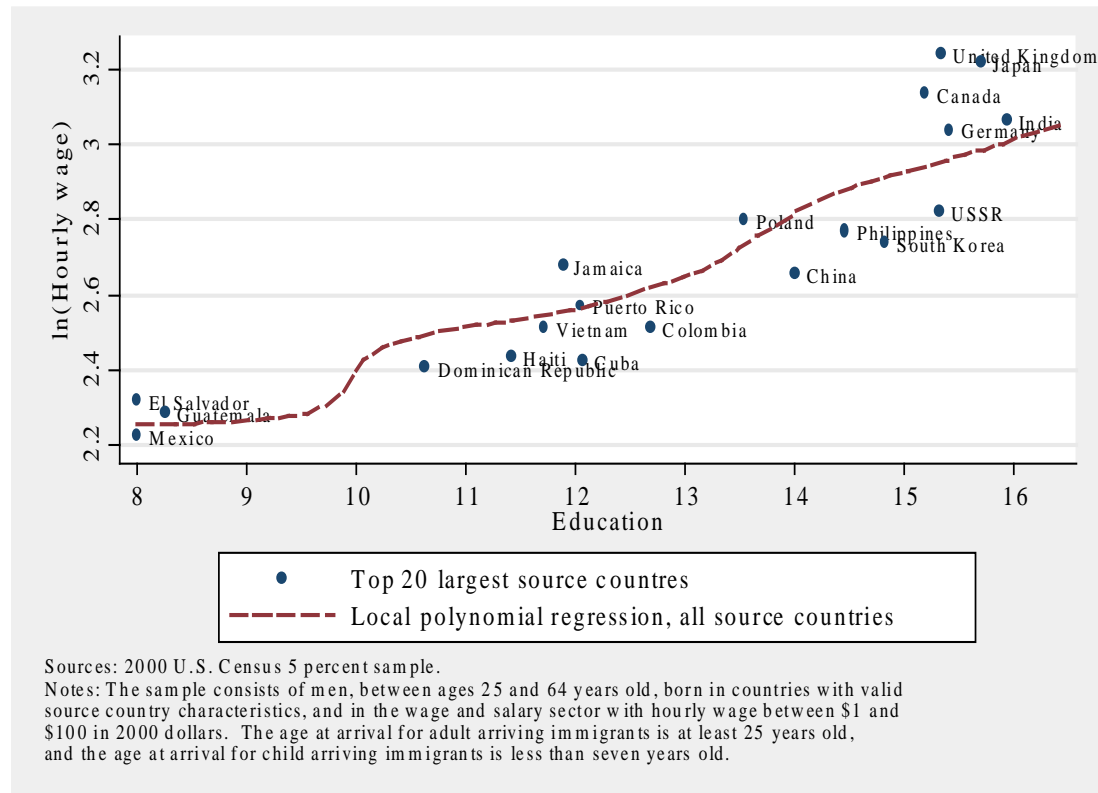


Figure 6. Country-Level Education among Adult-Arriving Immigrants

The country characteristics for the two groups show that child-arriving immigrants are more likely to come from countries with greater potential measures of school quality. The GDP per capita for adult-arriving immigrants is 5,300 dollars compared to 6,200 dollars for child-arriving immigrants. The average primary education expenditures per pupil as a share of GDP per capita and student-teacher ratio are roughly the same for both groups. Lastly, the labor force quality measure developed by Hanushek and Kimko (2000) is higher among the child-arriving immigrants than the adult-arriving immigrants. These differences are likely due to the

shift in the distribution of source countries from Europe to Asia and Latin America over the latter half of the twentieth century.

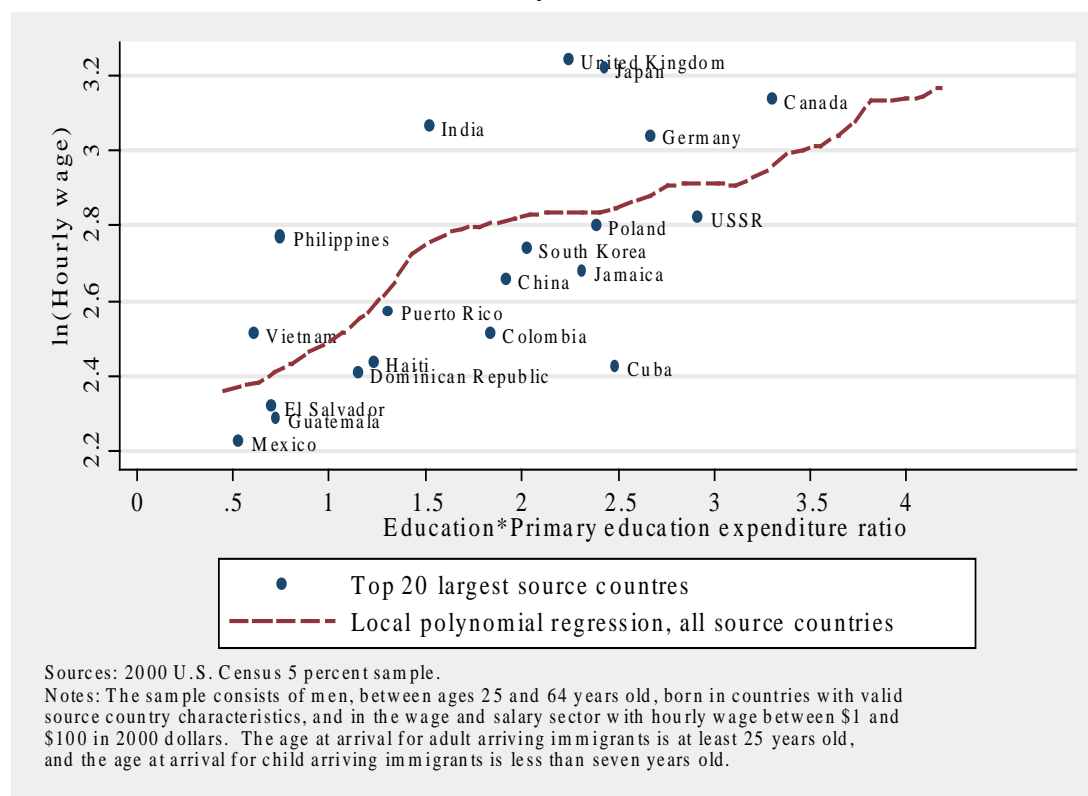


Figure 7. Country-Level Primary Education Expenditure Ratio-Adjusted Education and Wages among Adult-Arriving Immigrants

How do immigrant wages vary with the source country characteristics? I first present local linear regressions of country-average wages and country-average education in Figure 6 for the adult-arriving cohort. The scatter plot reveals substantial variation in average educational attainment, ranging from Mexico with about 8 years of education to India with almost 16 years of education. The slope of the line is generally positive, which suggests that the return to foreign education is positive.

The following three figures show the non-parametric relationship between wages and education after adjusting for potential measures of school quality. In Figure 7, the x-axis is the product of education and the primary education expenditure

ratio. There continues to be variation across countries for both wages and adjusted education, and the overall trend is positive. Figure 8 presents similar estimates after adjusting education by the natural log of the student-teacher ratio. The prior is a negative relationship since smaller class sizes are expected to provide students with greater student achievement. While the local linear regression does not have a consistent slope, it is generally positive, which goes against the prior. Lastly, Figure 9 shows the relationship between wages and education with a labor force quality adjustment. Again, the general relationship is positive, although the slope would be steeper if not for the presence of India as an outlier.

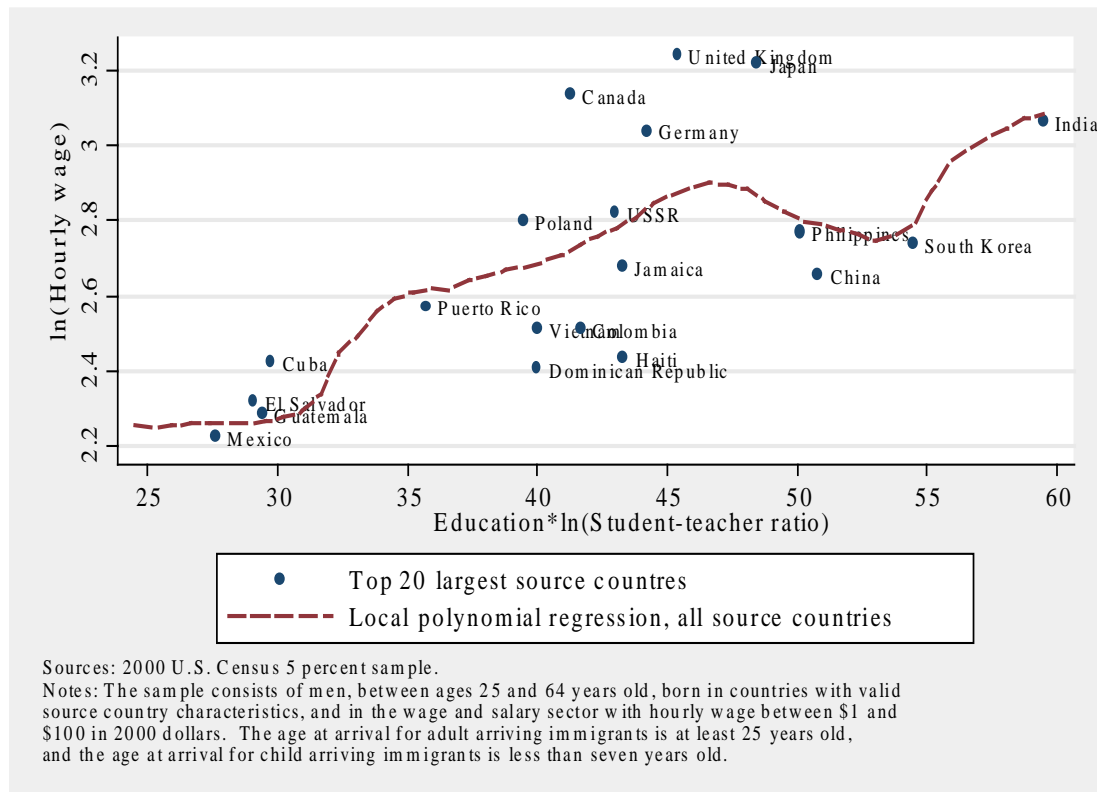


Figure 8. Country-Level Student-Teacher Ratio-Adjusted Education and Wages among Adult-Arriving Immigrants

The four figures provide preliminary evidence that the return to education could increase with the primary education expenditure ratio and with labor force

quality. Surprisingly, I show that it increases with the natural log of class size, when we typically expect smaller classes to be associated with greater school quality. However, since the specifications do not control for other important variables – both observable and unobservable – that vary among immigrants, I turn next to parametric analyses to hold these factors constant.

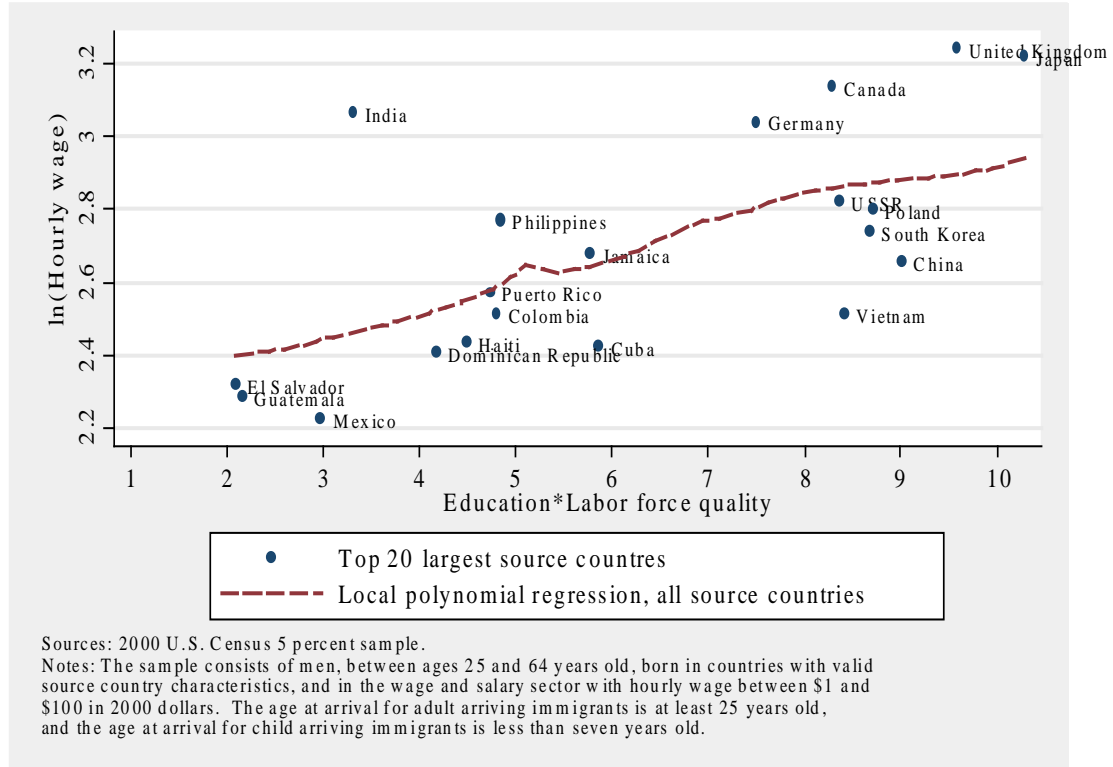


Figure 9. Country-Level Labor Force Quality-Adjusted Education and Wages among Adult-Arriving Immigrants

B. Research Design

The parametric research design is based on the standard regression of wages on education, school quality, the interaction between education and school quality with a set of exogenous control variables. For a discussion of the human capital models that rationalize this specification, see Heckman, Layne-Farrar, and Todd (1996). Equation (17) shows the baseline specification for individual i who was born in country c , in cultural group g , and resides in state s . I include years since migration and age at

arrival as exogenous control variables in \mathbf{X}_{icgst} . Because labor force quality and primary education expenditure ratios are time-invariant, I cannot use a country fixed effects specification. Instead, I account for the correlation between the wage residuals at the country level by clustering the standard errors. Estimating a naïve specification with ordinary least squares will lead to downward biased standard errors and generate inappropriate inferences. To account for other common cultural unobservables between respondents, I include race fixed effects as a rough proxy. I will also experiment with using source country language and region of the world fixed effects as alternative ways of holding culture constant.

$$\ln w_{icgt} = \beta_0 + \beta_1 E_{icgst}^f + \beta_2 Q_{cgt}^f + \beta_3 (E_{icgst}^f * Q_{cgt}^f) + \mathbf{X}_{icgst} \boldsymbol{\gamma} + \mathbf{Z}_{st} \boldsymbol{\pi} + g + t + \varepsilon_{icgst} \quad (17)$$

$$\ln w_{icgt} = \delta_0 + \delta_1 E_{icgst}^f + \delta_2 Q_{cgt}^f + \delta_3 A_{icgst} + \delta_4 (E_{icgst}^f * Q_{cgt}^f) + \delta_5 (E_{icgst}^f * A_{icgst}) + \delta_6 (Q_{cgt}^f * A_{icgst}) + \delta_7 (E_{icgst}^f * Q_{cgt}^f * A_{icgst}) + \mathbf{X}_{icgst} \boldsymbol{\gamma} + \mathbf{Z}_{st} \boldsymbol{\pi} + g + t + \varepsilon_{icgst} \quad (18)$$

As discussed above, the primary concern with this approach is that there are unobservables that vary by country and are correlated with school quality. Thus, I use the alternative specification presented in Equation (18) – the identification of the return to quality of education comes from variation across countries within cultural groups. I use first generation immigrants with younger ages at arrival as a counterfactual in that they should not be affected by school quality per say. δ_4 yields the common effect of potential school quality measure on wages, which reflects the effect of cultural unobservables. δ_6 indicates how the potential school quality measure affects the wage intercept for the adult-arriving immigrants, and lastly, δ_7 indicates the effect of the potential school quality measure on the return to education after removing the cultural unobservables.

A second approach is to use the two-step estimator by regressing first stage coefficients on aggregate characteristics (George J. Borjas 1987; Bernt Bratsberg and

Dek Terrell 2002; David Card and Alan B. Krueger 1992). The approach here is to separately estimate the return to education by age at arrival cohort and country. Separate regressions lead to a more flexible functional form than joint specifications that impose common effects of demographic characteristics on wages. I then take the coefficients on the return to education and regress them on the potential school quality measures, either with adult immigrants by themselves or with both age at arrival cohorts with interactions between the age at arrival cohort and the potential measures. Each country is weighted by the number of observations in the first step. Note that the latter specification is an improvement over previous studies because it differences out common cultural unobservables at the country level. This analysis will be completed in the future.

IV. Preliminary Empirical Results

Table 19 presents the wage regressions for the adult cohort, which matches the usual specification in previous studies. Since quality can either raises wages overall or for each additional year in school, I estimate models with and without the interaction between the potential measures of school quality and education. Column (1) presents the results for primary educational expenditure per pupil as a share of GDP per capita only. The return to foreign education is 5.7 percent, which is similar to previous estimates using other datasets. The return to the educational expenditure ratio is 0.724. The bracketed term in the table shows the standard error without correcting for arbitrary correlations within each country and indicates that the potential measure is associated with greater wages at a statistically significant level. In contrast, one I cluster the standard error at the country level, the coefficient estimate loses its precision and is no longer statistically significant.

In column (2), I repeat the specification but include the interaction between education and the primary education expenditure ratio. In this case, the interaction is

positive even after clustering the standard errors, which implies that the return to foreign education is greater among immigrants from countries that devote a higher share of their GDP per capita toward primary education. The remaining control variables are of the expected direction, with black, Hispanic, and other race immigrants earning less than white and Asian Pacific Islander immigrants. The return to an additional year in the United States is 1.8 percent, although this estimate is biased upward due to emigration bias (Darren Lubotsky 2007).

Columns (3) and (4) estimate the model that tests for the effect of the natural log student-teacher ratios. The sign of the quality measure without an interaction is negative in the naïve specification, which suggests that immigrants from countries with larger class sizes have lower wages. The interaction term in column (4) shows that this decrease grows in absolute value with each additional year in the United States. However, once I correct the standard errors, the estimates lose their precision and are no longer statistically significant.

The third set of results presents new evidence on the return to labor force quality among adult-arriving immigrants. The model includes a broader set of control variables than the specifications in Hanushek and Kimko (2000). Column (5) shows that labor force quality is positively associated with wages, both overall and with each additional year of education. However, as with the class size estimates, the results are no longer statistically significant once I cluster the standard errors at the country level.

The final set of results combines the three potential measures of school quality into a single specification. Column (8) shows that the return to foreign education is greater among immigrants from countries with greater educational expenditure ratios, even after clustering the standard errors. The other two potential measures are not robust to this correction, which suggests that only the educational expenditure ratio is possibly a meaningful measure of school quality.

Table 19. Variation in the Return to Education among Adult-Arriving Immigrants

	(1)	(2)	(3)	(4)
Education	0.057*** (0.011) [0.000]†††	0.035*** (0.012) [0.001]†††	0.057*** (0.011) [0.000]†††	0.106** (0.051) [0.004]†††
Primary education expenditure ratio	0.724 (0.644) [0.040]†††	-1.910*** (0.643) [0.098]†††		
Education*Primary education expenditure ratio		0.205*** (0.040) [0.007]†††		
ln(Student-teacher ratio)			-0.126 (0.128) [0.007]†††	0.062 (0.201) [0.017]†††
Education*ln(Student-teacher ratio)				-0.014 (0.017) [0.001]†††
Labor force quality				
Education*Labor force quality				
Years since migration	0.018*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††	0.018*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††
Age at arrival	-0.004 (0.003) [0.000]†††	-0.003 (0.003) [0.000]†††	-0.004 (0.003) [0.000]†††	-0.004 (0.003) [0.000]†††
N	145707	145707	145707	145707
R-squared	0.272	0.276	0.272	0.273

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: † p<.10, †† p<.05, ††† p<.01. The sample consists of men, between ages 25 and 64 years old, born in countries with valid source country characteristics, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. The age at arrival for adult arriving immigrants is at least 25 years old, and the age at arrival for child arriving immigrants is less than seven years old. All specifications also control for race, cohort, and year fixed effects.

Table 19 (Continued).

	(5)	(6)	(7)	(8)
Education	0.058*** (0.011) [0.000]†††	0.031 (0.030) [0.001]†††	0.056*** (0.011) [0.000]†††	-0.030 (0.060) [0.006]†††
Primary education expenditure ratio			0.592 (0.663) [0.045]†††	-2.335** (0.987) [0.122]†††
Education*Primary education expenditure ratio				0.227*** (0.081) [0.009]†††
ln(Student-teacher ratio)			-0.123 (0.133) [0.008]†††	-0.305 (0.247) [0.022]†††
Education*ln(Student-teacher ratio)				0.012 (0.015) [0.002]†††
Labor force quality	0.037 (0.288) [0.013]†††	-0.882 (0.627) [0.044]†††	-0.143 (0.326) [0.015]†††	-0.943 (0.602) [0.046]†††
Education*Labor force quality		0.065 (0.055) [0.003]†††		0.052 (0.051) [0.003]†††
Years since migration	0.016*** (0.002) [0.001]†††	0.016*** (0.003) [0.001]†††	0.019*** (0.002) [0.001]†††	0.019*** (0.003) [0.001]†††
Age at arrival	-0.003 (0.002) [0.000]†††	-0.003 (0.003) [0.000]†††	-0.004 (0.002) [0.000]†††	-0.003 (0.002) [0.000]†††
N	145707	145707	145707	145707
R-squared	0.270	0.273	0.273	0.280

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: † p<.10, †† p<.05, ††† p<.01. The sample consists of men, between ages 25 and 64 years old, born in countries with valid source country characteristics, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. The age at arrival for adult arriving immigrants is at least 25 years old, and the age at arrival for child arriving immigrants is less than seven years old. All specifications also control for race, cohort, and year fixed effects.

Table 20. Variation in the Return to Education among Child-Arriving Immigrants

	(1)	(2)	(3)	(4)
Education	0.081*** (0.005) [0.001]†††	0.067*** (0.007) [0.002]†††	0.081*** (0.005) [0.001]†††	0.141*** (0.030) [0.010]†††
Primary education expenditure ratio	0.122 (0.290) [0.080]	-1.746** (0.822) [0.276]†††		
Education*Primary education expenditure ratio		0.133** (0.050) [0.019]†††		
ln(Student-teacher ratio)			0.029 (0.019) [0.009]†††	0.274** (0.123) [0.042]†††
Education*ln(Student-teacher ratio)				-0.018* (0.009) [0.003]†††
Labor force quality				
Education*Labor force quality				
Years since migration	0.009*** (0.001) [0.001]†††	0.009*** (0.001) [0.001]†††	0.009*** (0.001) [0.001]†††	0.009*** (0.001) [0.001]†††
Age at arrival	0.017*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††
N	41007	41007	41007	41007
R-squared	0.212	0.213	0.212	0.213

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: † p<.10, †† p<.05, ††† p<.01. The sample consists of men, between ages 25 and 64 years old, born in countries with valid source country characteristics, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. The age at arrival for adult arriving immigrants is at least 25 years old, and the age at arrival for child arriving immigrants is less than seven years old. All specifications also control for race, cohort, and year fixed effects.

Table 20 (Continued).

	(5)	(6)	(7)	(8)
Education	0.081*** (0.005) [0.001]†††	0.050*** (0.014) [0.005]†††	0.081*** (0.005) [0.001]†††	0.079* (0.043) [0.014]†††
Primary education expenditure ratio			0.118 (0.305) [0.084]	-0.769 (1.193) [0.350]††
Education*Primary education expenditure ratio				0.065 (0.070) [0.024]†††
ln(Student-teacher ratio)			0.037* (0.021) [0.010]†††	0.139 (0.147) [0.050]†††
Education*ln(Student-teacher ratio)				-0.007 (0.010) [0.004]††
Labor force quality	0.117 (0.073) [0.028]†††	-0.866** (0.349) [0.143]†††	0.121 (0.073) [0.029]†††	-0.543* (0.314) [0.160]†††
Education*Labor force quality		0.068** (0.026) [0.010]†††		0.045* (0.023) [0.011]†††
Years since migration	0.009*** (0.001) [0.001]†††	0.009*** (0.001) [0.001]†††	0.008*** (0.001) [0.001]†††	0.008*** (0.001) [0.001]†††
Age at arrival	0.017*** (0.003) [0.001]†††	0.017*** (0.003) [0.001]†††	0.018*** (0.003) [0.001]†††	0.018*** (0.003) [0.001]†††
N	41007	41007	41007	41007
R-squared	0.212	0.213	0.213	0.214

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: † p<.10, †† p<.05, ††† p<.01. The sample consists of men, between ages 25 and 64 years old, born in countries with valid source country characteristics, and in the wage and salary sector with hourly wage between \$1 and \$100 in 2000 dollars. The age at arrival for adult arriving immigrants is at least 25 years old, and the age at arrival for child arriving immigrants is less than seven years old. All specifications also control for race, cohort, and year fixed effects.

Table 21. Triple Difference Estimates of Potential Measures of School Quality

	(1)	(2)	(3)	(4)
Education	0.078*** (0.007)	0.059*** (0.008)	0.076*** (0.007)	0.159*** (0.038)
Adult cohort	0.425*** (0.075)	0.433*** (0.078)	1.177** (0.487)	1.660** (0.795)
Education*Adult cohort	-0.020*** (0.007)	-0.023** (0.009)	-0.018*** (0.006)	-0.052 (0.057)
Primary education expenditure ratio	-0.413 (0.439)	-2.794*** (1.018)		
Adult cohort*Primary education expenditure ratio	1.174* (0.694)	0.888 (0.823)		
Education*Primary education expenditure ratio		0.174*** (0.056)		
Education*Adult cohort* Primary education expenditure ratio		0.032 (0.053)		
ln(Student-teacher ratio)			0.058 (0.050)	0.385* (0.196)
Adult cohort*ln(Student-teacher ratio)			-0.192 (0.143)	-0.329 (0.227)
Education*ln(Student-teacher ratio)				-0.024* (0.012)
Education*Adult cohort*ln(Student-teacher ratio)				0.010 (0.017)
Labor force quality				
Adult cohort*Labor force quality				
Education*Labor force quality				
Education*Adult cohort* Labor force quality				
N	186714	186714	186714	186714
R-squared	0.274	0.278	0.274	0.275

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: See Table 19 and Table 20.

Table 21 (Continued).

	(5)	(6)	(7)	(8)
Education	0.077*** (0.008)	0.028 (0.020)	0.059*** (0.011)	0.052*** (0.011)
Adult cohort	0.419** (0.157)	0.092 (0.272)	0.830 (0.497)	1.854* (0.978)
Education*Adult cohort	-0.017*** (0.006)	0.005 (0.023)		-0.082 (0.058)
Primary education expenditure ratio			-0.007 (0.438)	-2.832*** (0.993)
Adult cohort*Primary education expenditure ratio			0.548 (0.522)	0.473 (1.006)
Education*Primary education expenditure ratio				0.190*** (0.051)
Education*Adult cohort* Primary education expenditure ratio				0.038 (0.077)
ln(Student-teacher ratio)			0.038 (0.033)	
Adult cohort*ln(Student-teacher ratio)			-0.157 (0.131)	-0.302 (0.243)
Education*ln(Student-teacher ratio)				0.001 (0.002)
Education*Adult cohort*ln(Student-teacher ratio)				0.011 (0.015)
Labor force quality	-0.113 (0.170)	-1.653** (0.657)	0.039 (0.117)	
Adult cohort*Labor force quality	0.176 (0.312)	0.814 (0.657)	-0.163 (0.250)	-0.868 (0.584)
Education*Labor force quality		0.107*** (0.037)		-0.001 (0.007)
Education*Adult cohort* Labor force quality		-0.043 (0.050)		0.050 (0.050)
N	186714	186714	186714	186714
R-squared	0.272	0.275	0.274	0.281

Sources: 2000 U.S. Census 5 percent sample and 2005 American Community Survey.

Notes: See Table 19 and Table 20.

As a falsification test, I re-run the previous analysis on immigrants in the child-arriving cohort. Note that these children are educated in the United States and should

not be affected by school quality. The potential measures of school quality should only affect wages to the extent that they reflect cultural unobservables shared by all people born in the same country. Table 20 shows that the return to education increases with the education expenditure ratio even after clustering standard errors by country. These results suggest that the measure partially reflects cultural unobservables that are correlated with school quality and also with wages. It is promising though, that the size of the coefficient is 0.133, which is less than the corresponding coefficient for the adult-arriving cohort at 0.205. Under the assumptions of the identification strategy, the difference between these two estimates is the effect of school quality as measured by the educational expenditure ratio on the return to education.

The tests for class size and labor force quality reject both as meaningful measures of school quality. Class size has a negative effect on the return to education that is marginally statistically significant, which suggests that it reflects cultural unobservables. Note that the difference between the adult-arriving and child-arriving cohorts is actually positive, which would suggest that larger class sizes are associated with greater wages in the United States labor market. Columns (5) and (6) repeat the exercise for labor force quality, and again, the relationship between the potential measure of school quality and wages is stronger for immigrants who never went to those schools than immigrants who actually did. The fully specified model in Column (8) includes all potential measures and shows that the educational expenditure ratio and class size are not correlated with wages, but labor force quality is.

The final step is to estimate the triple difference specifications to separately identify the effect of shared cultural unobservables and the residual effect of school quality on the return to education. Overall, the results in Table 21 do not support any of the three country characteristics as meaningful measures of school quality. In

column (2), the education expenditure ratio is positively associated with an additional year of education for both immigrants who attended school abroad and immigrants who only attended school in the United States. This suggests that the measure should be interpreted as cultural unobservables that are associated with the return to education are not measures of school quality. Note that the interaction of education and the cohort dummy variable shows that the return to foreign education is less than the return to domestic education. The results for class size and labor force yield are similar, with each of the two potential measures affecting both cohorts equally.

V. Conclusion

What happens to the relationship between potential measures of school quality and the return to foreign education after removing the independent effect of cultural unobservables? The results in this paper suggest that primary educational expenditure per pupil as a share of GDP per capita, student-teacher ratios, and labor force quality do not measure the effect of school quality. Rather, they reflect cultural unobservables that are correlated with these both these measures and with wages in the United States. Hanushek, Rivkin, and Taylor (1996) who warn against using state-level characteristics as measures of local school quality. Perhaps unsurprisingly, this critique applies to country-level characteristics as measures of foreign school quality as well.

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